

NEW EXPOSITION
OF
THE FUNCTIONS OF THE NERVES.

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A
NEW EXPOSITION
OF THE
FUNCTIONS OF THE NERVES.

BY
JAMES WILLIAM EARLE.

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TO
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PROFESSOR OF ANATOMY AND SURGERY TO THE ROYAL COLLEGE OF SURGEONS,
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EVER FOREMOST AMONG THE
ZEALOUS AND SUCCESSFUL
IN PROMOTING IMPROVEMENTS IN

Medical Science,

THE FOLLOWING PAGES ARE INSCRIBED,

WITH EVERY SENTIMENT OF

ESTEEM AND REGARD,

BY

HIS PUPIL AND RELATIVE,

JAMES WILLIAM EARLE.

14, OLD BROAD STREET,
August 12, 1833.

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INTRODUCTION.



THE hecatombs of animals which have been sacrificed at the shrine of physiology, prove the great desire which has at all times impelled men to search into, and endeavour to elucidate the laws which regulate the vital functions of the animal economy, as well as the numerous difficulties they have met with in their attempts. The inquiry into these laws being a subject in itself so interesting to all classes of thinking men, it is not to be wondered at that it has absorbed so great a share of the study and attention of the many celebrated individuals, who have at various intervals been the ornaments of our profession; nor that the fame which they have each acquired, is in a great measure proportionate with the talent they have shown, and the industry and success with which they have prosecuted their physiological researches. One main inducement to so much

labour and perseverance, has been a conviction that each improvement in the scientific would be followed by its corresponding improvement in the practical part of our profession; for so long as we are unacquainted with the laws which govern the vital functions of our bodies during health, we must necessarily remain unable to comprehend, or to correct their deviations in the various diseases to which we are continually liable.

When we consider the many and almost insurmountable difficulties which present themselves to the physiologist in inquiring into the hidden functions of the nervous system—the various sources of error and perplexity ever occurring to the inquirer from injury to its functions by the very means employed to examine into them—how essential it is to the existence of animals, and how little light the mere knowledge of the anatomical structure and disposition of the brain and nerves has thrown upon the mode in which they operate in producing the different phenomena upon which the continuance of life depends, we can neither be surprised at the opposite conclusions at which some observers have arrived, nor at the unsatisfactory state of our knowledge up to the present hour.

Our acquaintance with diseases of the ner-

vous system may be considered as nearly parallel with that of our predecessors, with regard to those of the sanguiferous system before the discovery of the circulation of the blood by Harvey. They had some idea of the pulmonary circulation as we have of the different functions of the anterior and posterior spinal nerves; yet we wonder that they could have considered that the small amount of knowledge which they possessed, could enable them to account for phenomena of daily occurrence, and our successors, in like manner and with equal justice, will no doubt be surprised at us. But if the discovery of Harvey, admitting of such clear and satisfactory demonstration, met with violent opponents and a tardy acquiescence, in adducing facts which are addressed to the eye of reason only, I cannot expect to escape that share of prejudice and opposition which has always attended the announcement of novelty; nor that the opinions derived from the facts which I have brought forward will be readily allowed to be just; for, as it has been well observed by an old writer, "before truth in its silent or disputed march has roused the attention of the indolent, converted the supercilious, subdued the interested and obstinate, and reached the ears of all, an age has passed away."

The great alarm occasioned by the frequent occurrence of hydrophobia in the spring and summer of 1830, first caused my attention to be more particularly directed to the study of physiology. For as the history of the progress of medical science affords many instances of diseases having been intractable, until their nature was understood, I thought this was very probably the case with hydrophobia, and that if the cause of the violent spasmodic action of the muscles of the throat and chest could be ascertained, we might then know where and how to apply a remedy; and farther, that the want of this knowledge was the reason why all attempts to relieve it have hitherto been misdirected. In order to arrive at the cause of the improper action of muscles in disease, it was first necessary to understand the cause of their healthy action. This led me to undertake the examination of a multitude of experiments bearing upon this subject, and naturally involved the consideration of the laws which regulate the whole of the vital functions. But beyond inquiring into the cause of the healthy action of muscles, it was necessary to ascertain the nature of the power which enables them to contract at all; for it is as necessary that muscles should have a power of contraction, as that there should be a power to call them into ac-

tion, so that it may be useful for purposes connected with the maintenance and preservation of life.

Dr. Wilson Philip, in his valuable work on the Vital Functions, states as the results of experiments which he deems conclusive, that the contractility, or power of contraction of the voluntary muscles, is entirely independent of any influence derived from the nervous system, as is also that of all involuntary muscles, though these last are at all times readily influenced through that system. These facts, as he himself observes, seem to imply a contradiction, concerning which he says, "of this apparent inconsistency M. Le Gallois justly remarks, that two facts well ascertained, however inconsistent they may seem, do not overturn each other, but only prove the imperfection of our knowledge." The last of these facts, namely the readiness with which the involuntary muscles are influenced through the nervous system, is so evident to every one who has experienced fear, anger, or any strong mental emotions, that it might be supposed no person would venture to call it in question. It then occurred to me, that as it is not common to find any inconsistency in nature, and as a great deal of evidence is brought forward by Dr. Philip upon a point but little requiring it, the most important ques-

tion might have been overlooked. I therefore determined to ascertain whether there actually was any real inconsistency, and whether the facts were unobjectionable from which such opinions were deduced. In the pursuit of this inquiry I have been led to entertain many opinions as new as unexpected, and I have thought them sufficiently interesting to be submitted to the consideration of the profession.

In the first part of Dr. Wilson Philip's Treatise on the Vital Functions, the state of the evidence for and against the doctrine of irritability as taught by Haller is concisely stated. It there appears that up to the time of the commencement of the inquiries of M. Le Gallois certain objections had always been urged against this doctrine, which neither Haller nor any of those who had adopted his opinions, had ever been able to remove, and that each attempt to do this only involved them in greater difficulties. On this account Haller is rather to be regarded as having set up his opinions in opposition to those at that time generally received, and which attributed the action of muscles to something supplied to them from the brain by the nerves, than as having refuted them, and established his own. The experiments of Le Gallois were considered by a Committee of the French Institute as

having entirely overturned many of Haller's opinions, and also as affording a satisfactory explanation of several facts which no one had ever accomplished before him.

It has remained for the ingenuity of Dr. Philip to point out certain inferences which are not borne out by the facts adduced by M. Le Gallois. It is, in like manner, the object of the present investigation to shew that many of the facts brought forward by Dr. Philip do not warrant the inferences he has drawn from them. As Dr. Philip considers some of his experiments not only confirm some of the most important of Haller's opinions, but remove all objections which have ever been made to them, so in refuting the conclusions of Dr. Philip these objections must be allowed to return in all their force; consequently, the observation which he makes upon the experiments of Le Gallios, "by ascertaining some facts of great importance, while others immediately connected with them escaped his observation, have left the subject in greater confusion than he found it," may be applied with equal justice to his own. With one exception, which will be pointed out hereafter, there does not appear to be any reason to doubt the accuracy of Dr. Philip's experiments; but what I particularly wish to insist upon is, that the degree of value

to be attached to each, should be properly weighed. This is a point of the utmost importance, and is perhaps even more difficult than the devising what experiment is best calculated to afford the most decisive evidence on a particular question. So far indeed from doubting Dr. Philip's accuracy, it is chiefly because his experiments are well attested, and have been confirmed by others, that they will form the principal subject of the present examination. There is, however, a wide difference between bearing testimony to the truth or accuracy of a simple fact, and judging of the correctness of the inference deduced from it. In the one case any one who can see is competent, but in the other it is necessary that the witness should possess an extensive acquaintance with the nature of the subject of inquiry, as well as great discrimination and judgment, at least equal to that of the person instituting the experiment. My meaning is well illustrated by cases which frequently occur in the Courts of Law, in which a common jury is fully competent to decide upon the plain facts, while the more intricate and important points are reserved for the consideration of the judges. Dr. W. Philip has shewn that a Committee of the French Institute, composed of the most scientific men of France, has made serious

oversights in witnessing the experiments of M. Le Gallois. This being the case, it is within the verge of possibility that the gentlemen who were present at the experiments of Dr. Philip may have shewn the same want of discernment.

CHAPTER I.

Observations on certain Facts which have in a great measure given rise to the DOCTRINES of IRRITABILITY.

IF the details of Dr. Philip's experiments are read with that degree of attention, which is necessary to ascertain whether they warrant the inferences he has drawn from them, it will be readily perceived—

Firstly, That it is not proved that the power of contraction or Excitability of the Muscles of voluntary motion is dependent on the mechanism of the muscular fibre, and independent of the nervous system.

Secondly, That it is not proved that the action of the heart and arteries is independent of the nervous system.

Thirdly, That it is not proved that the nervous power is independent of the brain.

Fourthly, That it is not proved that the nervous power is identical with galvanism.

Fifthly, That it is not proved that the vital principle is independent of the brain.

So many of Dr. Philip's arguments are founded

upon the supposition of his having fully established these points, that the disproving of them will materially affect the greater number of his positions, and in the event of its being shewn that points so essential are not established, of course the arguments which rest upon them must fall. These arguments are so interwoven with the different parts of Dr. Philip's work, that if they are not valid the whole becomes immediately a mass of confusion in which it is extremely difficult to separate truth from error.

Dr. Philip has also fallen into a great mistake in confounding the "nervous power" with volition. As this is of importance in the effect it has upon his general opinions of the cause of the action of voluntary muscles, it is especially necessary that so great an error should be distinctly pointed out. He finds that salt when applied to these muscles excites contractions in them, whether their nerves are divided or entire. Any stimulant has the same effect; contractions are excited upon its application, and cease upon its removal. This Dr. Philip considers to be sufficient ground for concluding, that the nervous power excites the action of voluntary muscles. Any simple person might have supposed the salt was the stimulant; but no, Dr. Philip says it was the

nervous power. Are we then to suppose that this power takes upon itself the character of a stimulant only when the salt is applied? This would evidently be absurd, therefore Dr. Philip says the nervous power is always the stimulant to the voluntary muscles. Now if he is right, the power of volition, which is common to all animals capable of locomotion, is of no use at all. Every one who has paid attention to this subject must be aware that the muscles of voluntary motion never act during health, except when excited by the will; when they act involuntarily, such actions have very properly been considered to be morbid, as for instance those constituting the disease termed St. Vitus's Dance. This being the case the will is of use, and as we know that it is sufficient to cause every variety of action of which the muscles are capable, there is no necessity to suppose that there is any other stimulant of whose influence we are not conscious. Dr. Philip's conclusion therefore, that the nervous power acts as a stimulus to the muscular fibre, is entirely gratuitous; the utmost extent of the evidence afforded by the application of salt proving no more than that "the flesh will quiver where the knife is driven," and, that although the nerves are entire, the will has no power of preventing the contractions of mus-

cles when a stimulant is applied to the nerves, which are every where spread throughout their substance. This mistake of confounding the the nervous power with volition leads to great confusion with regard to what he has called the sensorial power, of which he considers volition as forming a part. The term *sensorium commune*, which has been applied to the brain, seems to have been the cause of Dr. Philip's having made use of the expression sensorial power; but whether he wishes to signify by this term the united functions of the whole brain, including the intellectual and moral faculties; or whether he only signifies volition, sensation, and that influence of the brain which secreting surfaces lose when their communication with this organ is interrupted by the division of their nerves, as three joined in one; or whether he means that they are independent of each other, it is absolutely impossible for the most minute attention to discover. But whatever meaning Dr. Philip attaches to the term sensorial power, it is certain that its two most obvious properties, namely, volition and sensation, bear an opposite relation to each other; the one being an active, and the other a passive property. It is not a little surprising that Dr. Philip, who has paid considerable attention to the relations

which the different parts and functions of the nervous system bear to each other, should have taken no notice whatever of the opposite relation which volition bears to sensation. Besides this opposite relation, these properties are entirely independent of each other; because volition may be exerted spontaneously at any time without any distinct external impression having been previously conveyed to the brain along the posterior nerves; nor is the perception of any such impression of necessity followed by an exercise of volition. On this account the use of the term sensorial power is highly objectionable, and as there is no function performed in the whole body in explaining which it is necessary to speak of these properties as united, it ought to be discontinued.

These are but specimens of the errors and general confusion resulting from them, which reign throughout a great part of Dr. Philip's work on the Vital Functions. It is unnecessary to pursue them farther at present, because, as they will be pointed out when the experiments which gave rise to them are under consideration, much unnecessary repetition will be avoided. Although Dr. Philip's experiments will not be found to warrant many of his inferences, yet as they are nevertheless true, the evidence they afford may still be applied to an

useful purpose, provided it is strictly adhered to. It will therefore be necessary to examine them carefully; for the same minute attention to his facts which leads to the detection of the errors in his inferences, will also be the means of distinguishing the truth. Previously, however, to entering upon this examination it is necessary to point out two important errors, arising from a most unaccountable misconception of well-known facts, which are of sufficient importance to affect the whole doctrine of Irritability; and then to give a short and comprehensive statement of the opinions which the study of these and other experiments has led me to adopt.

It appears to me that physiologists would have saved themselves a great deal of labour and disappointment had they properly considered in the first instance, and before they began their minute inquiries, 1stly, the circumstances which cause the life and death of an acephalous fœtus; and 2dly, the reason why the heart of an animal continues to beat a short time after the head is cut off.

The phenomenon presented by the birth of a brainless fœtus has generally been referred to, as containing the most positive and convincing evidence, that the doctrine, which teaches that the brain is the cause of all life and motion, is

erroneous. It has been supposed impossible to reconcile this doctrine with the numerous instances of these monstrous productions, which have from time to time been placed on record. It only, however, requires a little more attention, than has yet been paid to the circumstances attending them, to shew that instead of contradicting they actually confirm this doctrine.

In order to investigate this subject properly, the first question asked should be, what is the cause of the motion of the first punctum saliens in an embryo? The motion of the red globules of the blood is certainly an effect of some cause or other, for we cannot imagine them capable of moving by any inherent property. Is the nervous influence of the mother the cause? Of this we cannot be sure, but as there are, as yet, neither nerves nor brain in the embryo, or indeed any thing belonging to the embryo itself which can be the cause of the motion, the cause must be external to the embryo, and if the nervous influence of the mother be not that cause, it is impossible to know to what it can be attributed. A cause however must nevertheless exist, which, as it had the power of originating the motion of the first punctum saliens, so also, as *ce n'est que le premier pas qui coute*, must it have the power

to continue the motion of the blood until the fœtus is fully developed, and ready to be born; or in other words, capable of taking upon itself the maintenance of an independent existence. For this purpose it is necessary that the young animal should be provided with an apparatus calculated to supply exactly the loss it will sustain upon being separated from its mother; otherwise it must inevitably die upon being removed from that which had hitherto been the cause of its life and growth. Now the only system which appears at all fitted to answer this purpose is that of the brain and nerves. The next step in this inquiry is to ascertain whether a fœtus born with any very material defect in the system has ever been known to be capable of maintaining an independent existence. It is not necessary to examine and compare together every congenital defect or malformation of this system which has been observed, in order to answer this question satisfactorily. Almost every possible variety has been recorded, and the duration of their existence has invariably been proportionate to the approach towards perfection which the brain and spinal marrow had attained. Some, in whom there has been a spinal marrow and a small portion of brain, have been known to live even for a few weeks. Some, who have had

no brain but a tolerably perfect spinal marrow, have lived a few days; while those, who have had neither brain nor spinal marrow, have never breathed at all. It is extremely rare that such cases as these last are met with, in whom either heart, lungs, or ribs are perfect: Mr. Lawrence, indeed, in his excellent paper in the *Medico-Chirurgical Transactions*, to which I beg to refer the reader for farther information, says there is no recorded instance of a heart being found in a fœtus in whom no spinal marrow existed. Dr. Philip however, who is a later authority, says that such cases have occurred.

But whether they have or have not been born with a perfect heart and lungs, is of no consequence with reference to the present question; for whether they breathe or not, there is no difficulty in understanding why they should move immediately after birth, because being at that time only just removed from what had hitherto supported their growth and vitality, they are very nearly in the same condition as an animal from whose body these parts have been recently severed. Neither is there any difficulty in comprehending why the first two varieties should occasionally exist a few days or even weeks; because the nearer they approach perfection, however distant from it, the

better are they able to support and maintain for a short time the vitality which they possess at the time of birth; but such defective organization being insufficient to answer this purpose effectually, death must at length inevitably ensue. A fœtus therefore so circumstanced lives a short time, precisely for the same reason that the heart continues to beat a greater length of time when half the brain is removed, than when the head is cut off. Where then do we find in the circumstances attending the birth and death of these unhappy objects any evidence which proves that the brain is not the source of nervous influence? Do they not rather offer the strongest possible proof that unless there is an apparatus provided which is capable of supplying the place of that which is lost upon the fœtus being separated from the mother, it must inevitably die? The question is, not whether the cause which originated the motion of the first punctum saliens will continue to operate until the fœtus arrives at perfection, but whether it can support an independent existence without a perfect nervous system? Even if the brain and spinal marrow of a chick in its egg could be removed or destroyed without immediately destroying its vitality, such a fact would only prove that the functions of these organs do not commence until the time

of birth,* and that the nervous system of a fœtus has nothing to do with its growth while it remains in the uterus.

It is now, in the second place, to be inquired why the heart of an animal continues to beat a short time after the head is cut off, and whether this circumstance can be satisfactorily accounted for without supposing its action to be independent of the nervous system? The will, during perfect health, is always the power which calls the voluntary muscles into action, but it never has any power of either accelerating or retarding the contractions of the heart. Now when the head is cut off, the will, which was the cause of the action of the voluntary muscles, is removed at the same time; consequently, after a few contractions caused by the injury to the spinal marrow as the knife passed through it, they remain quiescent. The case however is very different with regard to the heart; for as neither the will, nor any exercise of intellect, ever had any effect upon the heart's contractions, the cause of its action is not removed when the head is cut off; consequently it ought not to be expected that the

* I understand that the dexterity of some French physiologist has accomplished this feat, and that the chick lived until the period of its being hatched arrived. I regret not having been able to learn the name of this adroit experimenter.

heart should cease to beat immediately upon the removal of that which never had any effect upon it. In a few minutes, however, it does cease to beat, and then stimulants so often fail to excite any farther contractions, that some have denied that it can be made to contract again. The heart is now said to be exhausted; but is this the case with the voluntary muscles? Certainly not; for as the cause of their action was removed with the head, they have remained at rest, consequently they are not exhausted, and are still capable of contracting when stimulated. Now, supposing that the nerves of these muscles and the nerves of the heart each had their proper proportion of influence at the time of the removal of the head, the nerves of the voluntary muscles and the nerves of the heart would be in an opposite state as regarded their nervous influence; the one would be plus, the other minus; consequently the voluntary muscles will contract when stimulated, but the heart will not: when however the former have been exhausted by having been repeatedly excited to contract, they are then, but not till then, in the same condition as the latter, and neither can be excited to any fresh contraction.

It appears to me impossible to arrive at any other conclusion from these observations than that the reason why the heart continues to

beat a short time after the head of an animal is cut off, is because the cause of its constant motion is not removed by such an injury. This fact therefore is not only equally well explained without supposing the heart's action to be independent of the nervous system, but much better, because such a supposition is totally at variance with what has just been said with respect to the evidence afforded by acephalous monsters, as well as by other facts which need not be noticed at present, as I shall have an opportunity of instancing them when speaking of the functions of the nerves by which the heart is supplied.

To the well-informed reader it will be a subject of curious and interesting reflection to observe, how the labours not only of Dr. Philip, but of Haller and his disciples have been misdirected, from their having considered the circumstances of acephalous monsters being born alive, and the continuance of the heart's motion after the removal of the brain and spinal marrow, as being incompatible with the opinion that the brain is the cause of vitality and motion, while a plain and simple statement of the facts connected with them clearly prove that they contain no contradiction. It is the more especially necessary that these two facts should be examined, as to whether or no they contain

any contradiction to the opinion that the brain is the source of nervous influence, because if they do not, as a contrary supposition has given rise to the doctrines of irritability, the advocates of these doctrines have all this time been striving to prove what was impossible; simply because their objections to the opinions respecting the agency of nervous influence had no real foundation. They do not however appear to have gained converts to their opinions so much from having established their own positions, as by reason of their opponents' inability to remove these stumbling-blocks, which have ever been cast in their way; for serious objections have always existed, which the doctrines of irritability have been incapable of explaining. One great object of Dr. Philip's work is to reconcile these difficulties, by endeavouring to prove that there are several distinct vital powers independent of, but bearing a certain relation to each other, I have already pointed out such errors as materially affect the foundations of his opinions; and if upon farther examination it be found that the ingenuity and perseverance he has displayed is unequal to the task, it may indeed be relinquished as utterly hopeless.

CHAPTER II.

*On the NATURE of NERVOUS INFLUENCE and on
the CLASSIFICATION of the NERVES.*

I SHALL now proceed to state the grounds upon which I consider myself justified in believing that a something, which has been called Nervous Influence, is constantly emanating from the brain; and then by classifying the several sorts of nerves according to the functions which each performs, to explain the mode in which this single power is made use of for the accomplishment of every purpose relating to the economy of animal life.

As there are many who deny the existence of any fluid which comes from the brain and passes along the nerves, on account of its being invisible; it is necessary to refer to the phenomena observed in certain fishes.

The chief object and great utility of comparative anatomy consists in our being enabled through its assistance, to observe the functions of parts which in some animals are largely developed, and thus to judge of the functions of similar parts which in other animals are less developed, and therefore less remarkable.

Certain species of fishes are observed to have the power of generating within their bodies a subtle fluid, which for want of a better term has been called animal electricity, although the points of difference between it and chemical electricity are more remarkable than their points of analogy. Animal electricity differs from chemical electricity, or galvanism, in not being evident to the eye by any display of light, nor to the ear by any explosive sound, it will not attract light substances, it will not charge a Leyden phial, it will not affect an electrometer, it will not chemically effect a change in any substance, and it will not pass through the smallest space: probably this last is the reason why similar phenomena are not observed in any land animal, because as it will not pass through the air, it could not be of the same use to them as it is to fish who live in a conducting medium. On the other hand, the points of analogy consist in its being only communicable through conducting substances, and the shock appears to be the same.

The circumstances connected with these remarkable fishes excited the attention of Mr. Hunter, who, after giving a description of the anatomy of the electric organs of the torpedo, makes the following observations. “The magnitude and number of the nerves bestowed on

these organs in proportion to their size, must on reflection appear as extraordinary as the phenomena they afford. Nerves are given to parts either for sensation or action. Now if we except the more important senses of seeing, hearing, smelling, and tasting, which do not belong to the electric organs, there is no part even of the most perfect animal, which, in proportion to its size, is so liberally supplied with nerves; nor do the nerves seem necessary for any sensation which may be supposed to belong to the electric organs; and with respect to action there is no part of any animal with which I am acquainted, however strong and constant its natural action may be, which has so great a proportion of nerves. If then it be probable that these nerves are not necessary for the purposes of sensation or action, may we not conclude that they are subservient to the formation, collection and management of the electric fluid; especially as it appears evident from Mr. Walsh's experiments that the will of the animal does absolutely control the electric powers of its body, which must depend upon the energy of its nerves.

How far this may be connected with the power of the nerves in general, or how far it may lead to an explanation of their operations,

time and future discoveries can alone fully determine.”*

When one of these fishes has been much irritated, the shocks which it gives gradually become so feeble as to be scarcely perceptible; but if he is allowed to remain quiet for a few hours, he is again capable of giving a very powerful shock. This fact proves that there is within the animal an apparatus capable of reproducing the electricity when it has been exhausted. It is impossible to suppose it can be restored by rest alone, for however long the animal might remain quiet, it would still be precisely in statu quo, unless some positive action had gone on *during* rest. Neither can it be imagined to have been reproduced by any action of the nerves themselves, because it cannot be supposed that this would have happened if their communication with the brain had been cut off by their division; but there is no difficulty whatever in attributing the reproduction of this fluid to an action of the brain, by means of which it may be gradually collected in the nerves; because such an opinion is in perfect accordance with so many well established facts which prove that the functions of the nerves are always affected in proportion

* Anatomical Observations on the Torpedo, by T. Hunter.—*Phil. Trans.* 1775, p. 486.

to any extent of injury which may be committed upon the brain; and if formed by the brain it can arrive at the electric organs by no other conductors than the nerves belonging to them.

If there are any who still deny the possibility of there being a fluid which comes from the brain and passes along the nerves, because it cannot be rendered evident to the sight, of them it may be demanded, what difference does the eye perceive between oxygen and nitrogen gas, or between hydrogen and carbonic acid? Neither of these gases have the same properties, though they are all equally fluid and invisible. Away then with the puerile objection of those who ask, who ever saw nervous influence? Is the caloric visible in the mercury of a thermometer when its column stands at the boiling point?

Our astonishment at finding such wonderful phenomena in the electric fishes may perhaps suffer some diminution on observing the power which some insects have of producing light, and which appears to be quite as much subject to their volition; but although this interesting point has not yet been fully investigated, there can be, I think, but little doubt of its being equally dependent upon some action of their nervous systems.

Thus the great difficulty of supposing the brain capable of forming any thing which can pass along the nerves, but which cannot be seen, and can only be appreciated by observing certain phenomena which indicate its presence, is entirely overcome. Whether the brains of all fishes form the same fluid in a much less quantity, or whether that which is formed in the brains of quadrupeds is the same as that of fishes, or whether either are identical with galvanism, are points of no consequence whatever as regards the present question, which is simply whether any thing does or does not come from the brain and pass along the nerves. No fact with which I am acquainted goes farther than proving this fluid to be *somewhat analogous* to galvanism; they all fall far short of proving their identity. Even the very experiments upon which Dr. Philip relies with so much confidence as completely deciding the question, admit of an interpretation totally different to his own. As however this question will be considered in another part of this work, it will be sufficient at present to observe that in every instance where a comparative experiment was made by dividing the pneumogastric nerves in two animals, one being galvanized and the other left to itself, the galvanized animal invariably died some hours before the

other. Is this fact compatible with the idea that galvanism restores that which has been lost by the division of the nerves?

It is now generally allowed by physiologists that the influence of the brain is necessary to secretion, although the precise mode of its operation has never yet been satisfactorily explained. There is great reason to believe that the whole secernent function, comprehending every thing relating to the growth and nutrition of the body, and particularly that part of it which constitutes the digestive process, is performed at least as well during sleep if not better than when the body is in motion. The action of the brain therefore by which this function is supported can never be at rest a single moment. As the life and health of the body depends upon the continuance of the secernent function, which is itself maintained by an action of the brain, I think the performance of this action by the brain ought to be henceforth distinguished by the term, *secernent function of the brain*. I consider this secernent function to be the result of an action of the whole brain for two reasons, first, because it is proved by Dr. Philip* that the action of the heart cannot be affected by a stimulant, however intense, if

* Experimental Inquiry into the Laws of the Vital Functions.

it be applied to a small part only, but the motion of this organ is immediately increased when a stimulant is applied to any part of the brain, provided the extent of the surface stimulated is considerable; secondly, the size of the brain is so small as compared with that of the body, that I think it is scarcely possible to be supposed that an action of a part only of this organ could be equal to the maintenance of all the vital functions. While the secernent function which maintains the health of the body is constant and uniform, and belongs to the whole brain, we have every reason to believe that volition and sensation, which provide for its convenience and welfare, belong only to particular parts of it, and are not always engaged. The first is therefore directly necessary to life, the two latter only indirectly necessary, because every function connected with the maintenance of life is perfectly performed during sleep when volition is suspended, and when no external impression is conveyed to the brain.

As the brain is constantly acting, it remains to be inquired in what manner this action is applied to fulfil its important office. The nerves are evidently the only means by which it can be rendered available; here of course, as in every inquiry of a similar nature, anatomy and

physiology must go hand in hand, for the most minute observation of the anatomical arrangement of the nerves could never have afforded any knowledge of the functions performed by them in living animals, unless recourse had been had to experiments upon them. The simple fact however obtained by the division of a nerve composed of many filaments, throws no light upon the precise mode in which those filaments had acted, nor does it shew whether all the filaments are of a similar nature; for instance, the division of the 8th pair of nerves in the neck is followed by a loss of the secreting power of the stomach, this is the result; but it does not inform us of the mode in which the nerves had acted upon the blood in the vessels of the stomach to produce secretion. In order to arrive at this, it is necessary to inquire how many sorts of filaments nerves are composed of, and then to ascertain what are their separate modes of action. This is so absolutely essential, that without it, however varied and multiplied experiments may be, they never can amount to more than an accumulation of facts, which no one can understand. This is, in a few words, precisely the state of physiology at the present time. Those who have formed theories and systems to account for every vital phenomenon, may perhaps

feel somewhat surprised or even indignant at this assertion, but it will be sufficient only to point at the many inconsistencies of these systems, not only with those of others, but with themselves, to prove its correctness; it is indeed more than an assertion, it is a truth to which the unsettled state of the minds of the whole profession, upon all the chief questions of physiology, alone bears ample testimony. The industry of our predecessors has left us but little to wish for as regards the plain facts; what we are now so much in want of is a clue to understand those which are generally regarded as certain truths. The good, which might have been done by their discoverers having been contented to register them as simple facts, has been in a great measure counteracted by the difficulties which their contending theories have thrown in the way of their successors; for each discoverer of a few new facts has had a certain number of disciples according to the plausibility of his doctrine, and the extent to which it has been propagated; it has thus been rendered incumbent on those who have perceived their errors to disprove them before they could establish their own positions. This might have been a warning to some who have felt the inconvenience, and have taught them that it was possible that those opinions,

which they fancied were founded on facts, were in reality based on nothing more solid than their own suppositions.

The only clue which can be made use of in unfolding the mysteries of the functions of the nervous system, is to be found in the discoveries of Sir C. Bell and M. Majendie, in which the distinct and separate functions of the nerves have been clearly demonstrated; it is not difficult therefore to comprehend how it has happened that those who commenced their labours before the facts contained in their experiments were known to the world, should have failed to explain and draw such inferences from their experiments as would have ensured general conviction and assent. Dr. Philip is the latest physiologist who has laboured under this disadvantage; and considering the difficulties he had to contend with, instead of imputing blame to him for errors which were unavoidable, we ought rather to be thankful to him for having done so much. These discoveries of Sir C. Bell and M. Majendie have been a great stimulus to the exertions of physiologists, and particularly to those of France; but no one has hitherto perceived the use that may be made of them, or the new views to which they give rise. A difficulty has evidently been felt to a certain extent, arising from the want of a proper

distinction between different nerves, which has as yet however only led to their being separated into two great divisions. The following is instanced as one of the latest which has appeared. "The* nerves may be divided into two classes: those which proceed directly from the brain and spinal marrow to the parts to which they convey the influence of these organs; and those which enter such ganglions as receive nerves proceeding from different parts of the brain and spinal marrow, whether these nerves have or have not protuberances belonging to themselves which have also been termed ganglions, but which receive only the different fibres that belong to the particular nerves to which they are attached, and from the circumstances in which they are placed, must have a different or at least a more confined relation to other parts of the nervous system." After giving Scarpa's description of a ganglion, from an unpublished lecture of Mr. Brodie, it continues, "the term ganglionic nerve I shall confine to those nerves which either enter or proceed from such ganglions, without adverting to their having or not having protuberances resembling ganglions belonging to themselves,

* Dr. Philip's Observations on the Functions of the Nervous System. Phil. Trans. 1829, p. 262.

although it is probable that a more perfect knowledge of the nervous system will point out this circumstance as a proper basis for a subdivision. It is necessary to keep this explanation in view, because neither the term ganglion or ganglionic nerve has hitherto been employed with much precision." The respiratory class of Sir C. Bell, which is another division, will be considered in its proper place, but it contains such a strange confusion of all sorts of nerves that it cannot conveniently be noticed at present.

The most cursory glance at the above-quoted classification is enough to shew its inefficiency as well as the inaccuracy to which it leads. In the first division, nerves of the special senses, composed of only one sort of filaments and which convey no influence *from the brain*, but simply convey external impressions *to the brain*, are classed with the fifth and spinal nerves, which are nerves of common sensation and of muscular motion, and are moreover composed of two sorts of filaments; and these again with the muscular nerve of the face, which receives branches from the superior cervical sympathetic ganglion; this classification, therefore, which seems only to have been suggested by their being connected with the brain;

and without the least regard to their natural functions, cannot be admitted. As this classification so evidently falls far short of what is necessary, and is rather calculated to perpetuate errors than to render the study of the functions of the nerves less intricate, I venture to propose that their different functions, according to what is at present known of them, should form the basis of their classification, and that they should be considered, first, according to the functions which each performs independently of the others, and secondly, according to their natural arrangement and the functions which they perform in combination. I do not propose this natural classification of the nerves merely on account of its novelty, but on account of the real advantages which it possesses. It will be found to render that which has hitherto been enveloped in the darkest obscurity at once clear and satisfactory; it will also introduce a degree of precision hitherto unknown in physiology, and thus remove the discredit into which this branch of medical science has fallen. It will moreover enable us to verify an opinion of Mr. Hunter, who, at page 188 of his *Observations on the Animal Economy*, says, “ that the origin and number of nerves being invariably destined for particular uses, I am persuaded

this general uniformity in course, connexion and distribution answers some purpose superior to mere mechanical convenience;" and at page 213, " I have no doubt that if their physiology were sufficiently known, we should find the distribution and complication of nerves so immediately connected with their particular uses, as readily to explain many of those peculiarities for which it is now so difficult to account."

The brain is formed naturally into two great and well-marked divisions, the cerebrum being the anterior and the cerebellum the posterior, and in order to understand the relation which the spinal marrow bears to the brain, its anterior and posterior columns, though in close apposition, must be regarded as equally distinct. Although every nerve, with the exception of those belonging to the sympathetic ganglions, as well as the spinal marrow itself, may be truly said to belong immediately to the brain, as is shown by the convulsions excited in the voluntary muscles by irritating certain parts of it, and also by the quickness with which external impressions are conveyed to it, yet they have with universal consent been named according to the parts with which they are more directly connected.

In unison with my plan of arranging the nerves according to their separate functions, I

propose, that the term cerebral nerve, instead of as hitherto being applied to every one which passes through an aperture in the skull, should be restricted to the olfactory, opthalmic and auditory nerves, on account of their being more particularly connected with the intellectual functions which are now generally regarded by physiologists as performed in some way or other by the brain, and from their being the most simple in their function, which is only that of transmitting impressions from their extremities to the brain, and from their filaments not being intermingled with those of any other nerve, that they should form the **FIRST CLASS**, and be considered as the only purely *cerebral* nerves.

That the **SECOND CLASS** should comprehend every nerve by means of which muscles are subjected to the influence of volition, these are the anterior nerves, and belong to the anterior columns of the spinal marrow. As this division embraces the third, the fourth, the anterior root of the fifth, the sixth, the portio dura of the seventh, and the ninth nerves, it will be seen that I have partially adopted the classification of Sir C. Bell, and, consequently, consider all the medullary fibres below the first formation of the crura cerebri, as comprised under the term Anterior Columns of the spinal

marrow, without regard to the circumstance of those fibres being encompassed at a particular part by other bands of medullary substance, termed *pons varolii*; or separated by other bodies which have been termed *corpora olivaria*.

That the **THIRD CLASS** should comprehend every nerve belonging to the Posterior Columns of the spinal marrow; thus including the posterior root of the fifth, the glosso-pharyngeal, the pneumogastric and Spinal Accessory nerves, all of which, with the exception of the last, transmit impressions from their extremities to the brain, and have protuberances upon them just before their respective junctions with the posterior columns of the spinal marrow; by the term Posterior Columns is signified their whole extent as far as the termination of the restiform bodies in the posterior peduncles of the cerebellum. The ganglions on these posterior nerves will, in the course of these observations, always be distinguished from those of the sympathetic by the term posterior ganglions; for the sake of avoiding any misunderstanding.

Lastly, that the **FOURTH CLASS** should comprehend every nerve proceeding from the sympathetic ganglions.

Every part to which branches of these nerves are distributed, whether its structure is muscular as that of the heart, or fibrous as that of

the arteries and iris, has a power of motion altogether independent of any influence that the will can exercise.

Such is the classification which I consider the distinct nature of the nerves obviously points out, and which I think is more particularly warranted by the well-known fact that nervous filaments however intermingled, whether united with others near to the brain and spinal marrow, or at a distance from them, never change their character. I must particularly beg the reader's attention to this arrangement, because it will enable him to understand more readily the observations I shall have to make respecting the functions of the nerves and the effects resulting from their combination.

These four classes of nerves are found distributed in four different orders.

The First Class, or purely cerebral nerves, are the most simple in their arrangement, because their filaments are never intermingled with those of any other nerve. These therefore should be considered as forming the **FIRST ORDER OF DISTRIBUTION**.

The next most simple arrangement is found in those nerves which are formed by the union of anterior and posterior filaments, or *the second and third classes*. These therefore should form the **SECOND ORDER**. These nerves supply

the bones, muscles, &c. of the limbs and a great part of the trunk of the body; and those parts, whose structure is such as to allow of motion, can at any time be called into action by the will.

The next variation is found in the union of posterior filaments with those belonging to sympathetic ganglions, or the *third and fourth classes*. These, therefore, should form the **THIRD ORDER**. All parts supplied by these nerves, whose structure is such as to allow of motion, such as the heart, arteries, intestines and iris, are observed to be in constant action from birth till death, without becoming exhausted or fatigued and without requiring rest. The action of these parts cannot be affected by the will.

The last variation is found in those nerves which are formed by the union of anterior, posterior and sympathetic filaments, or *second, third and fourth classes*. These, therefore, should form the **FOURTH ORDER**. The muscles supplied by filaments arranged in this order are observed to have a power of supporting constant motion without fatigue, but they differ from those supplied by the third order in being always obedient to the commands of the will. The nerves and function of the diaphragm afford an example of this variety. There are

several irregularities observed in the union of the filaments composing this fourth order. The intercostal nerves, for instance, are joined by filaments from sympathetic ganglions very near to the spinal marrow. The phrenic nerves again are joined by them at a greater distance from the spinal marrow. The anterior filaments which go to the muscles of the eye, are joined by sympathetic filaments just before their entrance into the orbit, but are not united to the filaments belonging to the posterior division of the fifth until they reach their respective muscles. The union of the portio dura with the fifth is also somewhat similar. These varieties, however, in the union of nervous filaments cause no confusion in the functions performed by them, because as nerves have no action except in their extremities, it is of no consequence whether they proceed in company to their destination, or whether they are joined just before their termination.

Of these orders of nerves it is to be observed, that those belonging to the first are single nerves, the second and third are double, and the fourth are treble nerves.

It is farther to be observed that while some parts receive anterior, some sympathetic, and others receive filaments of both these classes, the posterior filaments are common to all. The

reason of this will be found to be, that whatever difference of motion may be required in different situations and structures, the maintenance of the healthy state of the various tissues is necessary to all parts alike, in order that they may be in a proper condition to act; and for this purpose posterior nerves are indispensable.

I have as yet only mentioned the *nervus accessorius* or spinal accessory nerve, as an exception to the third class, and have refrained from a more particular notice of it until now, on account of its singularity. It is by means of this nerve that the trapezius, and mastoid muscles are enabled to assist in supporting and moving the weight of the head for a great length of time without becoming fatigued. Its communication with some branches of the spinal nerves is analogous to that of the phrenic with the branches from the inferior cervical sympathetic ganglion; therefore in classing nerves strictly, according to their functions and distribution, it ought to be considered as belonging to the fourth class, and fourth order; but it does not proceed from any ganglion, nor indeed has it any protuberance upon it, like the rest of the nerves belonging to the posterior columns of the spinal marrow. There is

another point of view in which this nerve becomes extremely interesting, for while the whole of the rest of the posterior nerves in conveying external impressions from their extremities to the brain, may be said to *terminate* in the posterior columns, the two Spinal Accessory nerves in supplying muscles with a power of acting constantly, appear to arise from these columns. It will be found however that this difficulty will admit of an easy solution, as soon as I have explained the mode in which, by means of the nerves, that which I have ventured to call the Secernent Function of the brain is applied to the maintenance of life and health. In this function the nerves of the first class, or purely cerebral nerves, take no part, those of the second, third and fourth classes only are concerned; to them therefore will my observations at present be confined.

As the mode in which all the functions of the brain and spinal marrow are performed will form the subject of a future treatise, the function of the first class, which have no relation except with those of the brain, may very properly be deferred till then. The consideration of the nerves which supply the organs of generation (although belonging to those of which I am now about to speak), is also postponed on

account of their being called into action by emotions of the mind, as also those of the iris which are shewn by the following experiments to have a strict relation to the functions of the brain.

“Exp. 4. When the optic nerves are pinched in the cranial cavity of a living pigeon, or immediately after its decapitation, the pupils are contracted for an instant on each injury of the nerves.

“Exp. 7. When the optic nerves have been divided within the cranial cavity of a pigeon immediately after its decapitation, if the portion of the nerves attached to the eyes be pinched no contraction of the pupil ensues: if the portion attached to the brain be pinched a like contraction of the pupil ensues as if the optic nerve had not been divided.

“Exp. 9. When the third nerves have been divided in the cranial cavity of the living or dead pigeon, no change in the pupil ensues upon irritating the entire or divided optic nerve.”*

On this account, although the secernent function of the brain is the result of a physical action in it, and though an act of volition and

* See Mayo's Commentaries, No. 2, page 4.

an impression upon the extremity of a posterior nerve are preceded and succeeded by an action of the brain, the nature of these actions will not be explained at present: nor will any notice be taken of the numerous experiments upon the brain, accumulated principally by the labour and industry of the French physiologists. These facts are like an immense quantity of stones lying in a quarry ready hewn in proper forms, and only awaiting the arrival of an architect who has but little more to do than to assign to each its proper place, in order to erect a splendid edifice. By the accomplishment of the second part, I shall have carried into effect my plan of giving a complete exposition of the whole functions of the nervous system. The division which I have thus been induced to make, will be found so distinct that no confusion will arise from different functions independent of each other being treated of separately, which when taken together will form a perfect whole. I have been led to adopt this mode of proceeding, not only on account of the extensive as well as complicated nature of the functions of the brain, but in order that the minds of the profession may be accustomed to consider those of the nerves first, in a point of view differing materially from any

which has hitherto been proposed, and by being freed from the doctrine of irritability, as proposed by Haller, and considered to be confirmed by Dr. Wilson Philip, which ever since its promulgation has been a clog and a fetter upon the progress of physiological science, they will thus gradually be more prepared to admit, and at the same time better qualified to judge of the truth of a system, which in so many particulars is altogether new. It will be sufficient for my present purpose to know that certain actions do take place in the brain, and to regard the spinal marrow simply as the conductor between the brain and the nerves, conveying its influence or power to every part where the extremity of a nerve is found, and through the intervention of which the muscles are subjected to the influence of the will, and also by which, external impressions are conveyed to the sensorium. For the reasons above mentioned those experiments, which seem to shew that the filaments of the anterior columns cross over and are intermingled with each other in their course down the vertebral canal, and that the same intermixture occurs in the posterior columns, will not be noticed, for however often they cross and recross, still if the one set originally *springs from the cerebrum*, and if the other eventually *termi-*

nates in the cerebellum,* the experiments contain no contradiction to the opinions which will be advanced respecting their conducting properties.

It now only remains that I should state the precise manner in which I consider the influence of the brain, or more properly the CEREBRAL INFLUENCE, because it proceeds from the cerebrum, and not from the cerebellum, is applied by the nerves in order to effect secretion, and to supply the voluntary and involuntary muscles with their power of contraction. The anterior and posterior nerves, or second and third classes, bear the same opposite relation to each other as has been already noticed with regard to volition and sensation, the one being active, the other passive or recipient. The action of the anterior nerves is always from the cerebrum towards their extremities, that of the posterior is exactly the reverse, being from their extremities towards the cerebellum. There is no instance in the whole body of an anterior nerve not being joined to a posterior nerve. This arrangement appears to be for the purpose of allowing the cerebral

* See Reil's Anatomy of the Brain, as translated by Mr. Mayo, and published in his Anatomical and Physiological Commentaries. London, 1828.

influence, which flows along the anterior, to return in a reflux direction along the posterior nerves and posterior columns of the spinal marrow to the cerebellum; thus completing a circulation as perfect as that which is carried on by the arteries and veins, and which I venture to call the **CIRCULATION** of the **NERVOUS SYSTEM**.

It appears to me that the passage of the cerebral influence, from the extremity of an anterior to that of a posterior nerve, offers a much stronger analogy to galvanism than any which has been pointed out by Dr. Philip, though I am still far from thinking it sufficient to establish their identity. It is to this passage of the cerebral influence, from the extremity of one nerve to that of another, analogous to the passing of galvanism from the positive to the negative pole, that I am inclined to attribute the maintenance of the blood in its fluid state, the evolution of caloric, the secretion of synovia in the joints and bursæ, and, in fact, the support of the whole secernent function upon which the deposition of new and the removal of old parts depends. It is farther to be observed, that as this circulation is constantly going on, there is always a certain quantity of cerebral influence in the anterior nerves, consequently all the muscles to which

these nerves are distributed must always act in obedience to the will, simply because volition can affect their prime origins, which are in the cerebrum.

In this simple arrangement, however, there is no provision for the supply of the viscera, but as their secernent and motor function equally require the presence of cerebral influence, there must be some contrivance whereby it may be supplied. For the accomplishment of this object, every posterior nerve is provided with a ganglion, (which as they do not interfere with the transmission of external impressions, evidently perform some office which has not hitherto been understood,) in order that part of the current which is returning in a reflux direction along the posterior nerves, upon which alone these protuberances are found, may pass at regular intervals into the sympathetic ganglions. How apparent the object of the wandering course of the pneumogastric nerve (which is entirely composed of posterior filaments at its termination, or what has hitherto been considered its origin,) now becomes! The secernent function of the viscera must be performed in the same way as in the limbs, and an opportunity is thus afforded by the pneumogastric nerves, for the passage of the same fluid from the extremities of the nerves coming

from sympathetic ganglions, to the extremities of nerves of an opposite character, at once providing for the whole secernent function of the viscera; the differences in the various secretions not resulting, except in some degree perhaps in the liver, from any difference in the blood itself or in the fluid which acts upon it, but from the peculiarity of the structure of each secreting organ. I have no words to express my admiration of the beauty of this arrangement, whereby the motion of all parts supplied by the third order of nerves is rendered both involuntary and unceasing: involuntary because the influence of the will cannot possibly extend beyond the extremities of the anterior nerves, and unceasing because that which bestows the power of motion is supplied in an uninterrupted stream. Are we now able to explain the reason why the heart has nerves? and why every variety of mental emotion has an effect upon secretion in proportion to its intensity?

The rest of the returning current, which is over and above what is necessary for the supply of the viscera, passes upwards along the posterior columns of the spinal marrow into the cerebellum, supplying in its passage the spinal accessory nerves, so that the muscles to which they are distributed are endowed with the

power of maintaining long continued action without fatigue. Thus the physiology of these nerves, which at first sight appears so difficult of explanation, becomes a most convincing and unexpected proof of the existence of a CIRCULATION IN THE NERVOUS SYSTEM. The superior cervical ganglion of the sympathetic is supplied from that on the pneumogastric precisely as the thoracic, abdominal, and pelvic sympathetic ganglions are supplied from the ganglions on the posterior nerves which are in their neighbourhood.

I have thought it right to make this statement of what my opinions are, previously to bringing forward the facts from which they are drawn, in order that the reader, by being made familiar with the points to be proved, may be better able to judge whether the evidence is sufficient to support them.

It will be found convenient to distinguish the fluid which is refluent along the posterior nerves from their extremities to the cerebellum by the term NERVOUS INFLUENCE. It is not intended by this expression to convey any idea that the CEREBRAL INFLUENCE undergoes any change in its passage from one class of nerves to another, but merely to express the reciprocal correspondence which exists between the brain and the nerves, and the mode in which the

sympathetic ganglions are supplied. After this explanation I shall not be misunderstood in saying, that the anterior nerves receive cerebral influence, and that the sympathetic ganglions receive nervous influence.

CHAPTER III.

*On the FUNCTIONS of the SECOND ORDER
of NERVES.*

IN examining the experiments of Dr. Philip, it will be convenient to take them in the following order:—first, that relating to the action of voluntary muscles; secondly, those relating to the action of involuntary muscles; thirdly, those relating to secretion; and to examine, in the last place, his theory of respiration; because I shall thus be enabled to consider the nerves of the second, third and fourth orders according to what I have stated to be their natural arrangement. No confusion can arise from considering the secernent function of the second and third orders together, because the mode in which secretion is effected, viz. by the passage of the cerebral influence from the extremity of one nerve to that of another, is the same in each. As Dr. Philip's experiments are now so well known, it will not be necessary to adduce more than a few of the most important of them; and it so happens that his 35th, which is the key and centre of his position, will be the first

to come under examination. This, which on account of its importance ought to have been his strongest, is in fact his weakest point. It is also of great consequence to my own position that the evidence afforded by this experiment should be duly weighed; because if Dr. Philip's conclusion is correct regarding the independent nature of the "muscular power," there can be no necessity for this power to be supplied in the manner I have stated in the preceding chapter.

I think it right to notice in this place that many physiologists appear to have erred very much in thinking that no structure save that of a muscle is capable of contracting, and that therefore every structure which contracts must necessarily be muscular: even Mr. Hunter has fallen into this mistake. There are other structures however which, although not muscular, or at least in which fibrin (which is always found in muscular substance) does not form a component part, are yet most undoubtedly endowed with a power of contraction; as, for example, that of the iris, of the uterus, and of the smaller arteries. Both the iris and uterus have been *supposed* to be muscular, because they have a power of contraction; some have denied that the smaller arteries possess this power, because muscular fibres have not been found to enter into their composition; others

again have asserted that they must be muscular, because they have a contractile power; but by asking whether it has been proved that the structure of muscle is that which is exclusively endowed with contractile power, the source of the error becomes at once apparent. The following observations may serve to illustrate this point. It is observed that steel, whalebone, and wood, a mineral, an animal, and a vegetable production, each possesses the property of elasticity in different degrees, and each is made use of according as the different degree of elasticity which it possesses is adapted to answer a particular purpose; yet no one thinks it necessary to suppose that steel must enter into the composition of whalebone, nor that whalebone exists in the structure of wood. Is it not therefore much more reasonable to consider that the structure of muscle, of the iris, of the uterus, and of artery, is that which is most proper to perform its allotted action? Of the contractile power of the iris and of the uterus there can be no doubt whatever; and the well known fact of the continuance of the motion of the blood in the capillary vessels for more than an hour after the excision of the heart, the act of blushing, the reddening of the wattles of the turkey cock, as well as another circumstance which immediately suggests itself,

all concur to place the contractile power of artery equally beyond all question, even though muscular fibres never have been and never will be found to form a component part of their structure.

The simple contraction of muscular fibres, whether called into action at intervals as in the voluntary muscles, or more frequently as in the heart, or more slowly as in the coats of the intestines, is everywhere alike. The structure also of the muscular fibre is the same wherever it is found. It is true there is some difference in their arrangement in various organs; but the only variation arising from this cause is in the degree of force which is the result of it. The purely voluntary muscles never act without being excited by volition, and soon become fatigued; the involuntary muscles, on the contrary, maintain a constantly repeated action, independent of the will, from birth till death, yet they never become weary. Here then is an instance of similarity of structure and dissimilarity of function.

In the heart and arteries, on the other hand, may be observed an example of a dissimilarity of structure with a similarity of function. If these circumstances be allowed their due share of consideration, it seems impossible to conclude that the structure of muscle, of artery,

or of any other part can be the cause of their respective modes of action, or can have any thing to do with it beyond being that which is best adapted to be acted upon by some cause which must be sought for elsewhere. The only clue which appears at all likely to lead to the unravelling of this mystery is to be found in the fact, that similar structures supplied by different nerves have a different mode of action, while dissimilar structures supplied by the same nerves perform a similar action, from which it seems to follow as a necessary inference, that all action, whether voluntary, involuntary, or partaking of both, is constantly and entirely dependent on their respective nerves.

These considerations, which have been altogether overlooked by Dr. Philip, are brought forward with the view of shewing the difficulty he had to contend with in endeavouring to prove that the excitability of muscle is a power inherent in their structure and in no way dependent on any influence derived from the nerves or nervous system. His 35th experiment was instituted to remove every objection which had ever been raised against this opinion, but more particularly to obviate the following, "that although the division of the nerves may

prevent the muscle from receiving more nervous power, it does not deprive it of that already bestowed on it, either forming a necessary part of its power, or dispersed through its substance in nerves too small to be removed; and this objection appears to be greatly strengthened by the muscle soon losing its excitability after it is separated from the body, and those *muscles whose function is supported by stimulants* peculiar to themselves being still supplied with nerves." *

I cannot avoid pausing to take notice of this expression, "muscles whose functions is supported by stimulants," because every fact with which we are acquainted shews that the power of muscles is *exhausted* by stimulants. Another objection is contained in the fact, that when animals are deprived of life by lightning or by any poison which suddenly affects and destroys the nervous system, the muscles cannot be stimulated to contract after death. To these I may be permitted to add there is no experiment with which I am acquainted which goes the length of proving that muscles when exhausted can recover their power after the division of their nerves. There is no scarcity of

* Experimental Inquiry, Vital Functions, third edition, p. 87.

experiments which prove that muscles can contract after the division of their nerves ; nor are we under the necessity of resorting to experiment to shew that muscles may be exhausted by frequent contractions while the nerves are *entire and uninjured* ; nor that muscles under these circumstances can and do recover their power during rest, because these are facts recurring daily both to ourselves and horses. The only point therefore requiring farther elucidation is, whether *muscles after being exhausted can recover their power when their nerves have been divided*.

The experiment which Dr. Philip has instituted to determine the question respecting the independent nature of the “muscular power,” is the 35th in the third edition of his work on the Vital Functions, but I have preferred quoting from the second edition, in which it is the 32d, on account of its being more circumstantially detailed. How fully Dr. Philip relies on the evidence which he thinks this experiment affords may be learned by observing how frequently in other parts of his various works he refers to it as the basis of his argument.

“ Exp. 32. All the nerves supplying one of the hind limbs of a frog were divided so that it became completely paralytic. The skin was

removed from the muscles of the leg and salt sprinkled upon them, which, upon being renewed from time to time, excited contractions in them for twelve minutes, at the end of this time, they were found no farther capable of being excited. The corresponding muscles of the other limb, in which the nerves were entire, and of which the animal had a perfect command, were then laid bare and the salt applied to them in the same way. In ten minutes they ceased to contract, and the animal had lost the command of them. The nerves of this limb were now divided, but the excitability of the muscles to which the salt had been applied was gone. After the experiment the muscles of the thighs in both limbs were found to contract forcibly on the application of salt."

"It is remarkable," he continues, "that in this experiment the excitability of the muscles whose nerves were entire was soonest exhausted;" and farther observes, "from this experiment it is evident, that the nervous influence, so far from bestowing excitability on the muscles, exhausts it like other stimulants. The excitability therefore is a property of the muscle itself." From this experiment it will be observed Dr. Philip draws two conclusions,

the first is that the *nervous influence* excited the muscles to contract; the second is that the muscular power is independent of the nerves. It farther appears that the last is rather a secondary inference from the first conclusion, than drawn from any direct evidence afforded by the experiment itself; because nervous influence cannot both exhaust and supply excitability.

It has been seen that contractions were excited in the muscles of both legs of the frog, and that the muscles were exhausted thereby. Now the first point to be decided is whether those contractions were excited by the nervous influence or by the salt. If it should appear that the salt was the stimulant in this experiment, it follows that the contractions excited by its application were performed at the expense of the nervous influence (it being allowed on all hands that a certain quantity of this influence remains in the nerves after their division); if this is the case, nervous influence and excitability are one and the same, and they are not separate and independent powers as Dr. Philip has imagined. I think the following question will place this subject in its true light—Is it probable that any contractions would have occurred in either sets of muscles if the

salt had not been applied to them? This will be best answered by considering what is the natural state of a voluntary muscle, and cause of its action. During perfect health a purely voluntary muscle is constantly at rest except when excited by volition. From this we learn that no positive exertion of the will is necessary to maintain the voluntary muscles in a state of rest, but simply that its power should not be exercised. We learn also from what is observed in disease, as in Chorea, in Cholera, and in Tetanus, that the will has not the slightest power of preventing the contractions of voluntary muscles. If these observations are correct, it is evident that muscles will contract when stimulated, whether the nerves are divided or not, but unless excited they remain at rest. The muscles of the frog's legs therefore would not have contracted if the salt had not been applied to the nerves, which are everywhere spread throughout their substance. This conclusion is farther strengthened by the impossibility of supposing that the nervous influence only takes upon itself the character of a stimulant at the time of the application of the salt.

If Dr. Philip still wishes to prove that the nervous influence excites the muscles, it will be necessary for him to prove that voluntary muscles will contract spontaneously after the

division of their nerves, which is an impossibility.

There is no difficulty in explaining why the muscles of the second leg were exhausted sooner than the first, because we must suppose each leg to have possessed an equal quantity of excitability at the commencement of the experiment, and as it appears that muscles are exhausted by contractions, however excited, we cannot but suppose that those which are excited by the will must exhaust them likewise. The struggles of the frog, therefore, which is by no means a patient animal under pain, during the first twelve minutes occupied in exhausting the first leg, would have exhausted a considerable part of the excitability of the leg over which the will retained perfect command before the salt was applied to its muscles. I am led to think this explanation correct, because when I fixed a frog in such a way as to prevent him from struggling, I could not perceive the difference noticed by Dr. Philip, but on the contrary, the muscles whose nerves were uninjured continued their contractions the greatest length of time. Upon taking all the circumstances into consideration, this conclusion of Dr. Philip's seems to be as marked an instance of a person being predetermined to make his experiments meet his views as can be met

with; he strips some muscles and sees that they remain at rest until he applies a powerful stimulant, and then, because they contract, he attributes their contractions not to the stimulant employed but to something else. As far as I am capable of judging, the cause of Dr. Philip's mistake lies in his having confounded the nervous influence with volition, and in having added to the confusion thus created by using the term nervous influence as signifying something distinct from excitability, but between which there is no perceptible difference. I am also of opinion, that if he had not allowed himself to go beyond the evidence contained in his experiment, he would have said, the will and the excitability are independent of each other; the latter is not bestowed upon the muscles at each exercise of volition, but is constantly resident in them, and enables them to support a certain number of contractions, whether excited by the will or by any powerful stimulant; and that the contractions excited by salt exhaust the muscles more completely than those occasioned by the will, since we cannot conceive it possible that voluntary motion could be continued so long, as to be attended with so great a degree of exhaustion as we see produced by the salt. We search in vain however, for any evidence to shew that

the excitability is independent of the nerves, as well as for the reason why it should be supposed to belong exclusively to the muscular fibre. I can even imagine it to be resident in this structure, and yet not independent of the nerves; as for instance, caloric is to a certain extent resident in the bars of a grate while the coals are burning, but we do not say that the caloric is independent of the fire because the bars remain hot for a time after the coals have been removed.

Dr. Philip observes that voluntary muscles contract with great irregularity when stimulated, and appears to be at a loss to account for such an occurrence. But this is precisely what we ought to expect, when we consider that the function of the will is to excite the muscles subject to its influence to any degree of contraction from the slightest motion up to the highest degree of tension they are capable of, and at the same time to regulate their contractions so as to effect the accomplishment of any given purpose, whether to guide a pen, or raise an hundred weight; but neither salt, nor any other artificial stimulus can be supposed to be capable of exercising any discretionary power over the muscles similar to that of the will, so as to regulate either the degree or order of their contractions. Therefore all contractions of the

voluntary muscles excited by artificial stimulants must necessarily be irregular, as well as all those which occur in the diseases above mentioned, in Chorea, &c. &c. because they also take place independently of the will. The difference between the regularity of the contractions excited by volition, as compared with those excited by salt, is remarkably well seen in the denuded leg of a frog,

In another part of his work Dr. Philip lays some stress upon the fact that the excitability of the muscles is unimpaired in apoplexy. Is there any thing wonderful in this? or is a person labouring under an attack of apoplexy in a worse condition than an animal whose head has been cut off, and in which the excitability of the muscles is also unimpaired?

Upon this 35th experiment Dr. Philip relies for the proof of the correctness of his conclusion; but it does not shew "that muscles can regain their power by rest after their nerves have been divided." This therefore is a mere assertion, which derives no confirmation either from the 35th or any other experiment hitherto published by Dr. Philip. We must not however forget that this last is the only point upon which any evidence was required, and nothing short of which could have set this long disputed question at rest for ever. This being the case,

it occurred to me that the only way of deciding it would have been to have preserved the aforesaid frog, and to have noted which of the two sets of muscles recovered their power of contraction. If this power had been observed to be restored in those whose nerves had been divided as well as in those whose nerves were entire, there would have been good reason for concluding that excitability is inherent in the muscular fibre; but I must again repeat that nothing short of proof to this extent can warrant any such conclusion. With this view the following experiment was performed.

Exp. A. It appeared that the exhaustion of both legs of a frog was a more severe injury than it could sustain. Two frogs were therefore chosen one larger and stronger than the other, because the one would be subjected to a greater injury than the other, and also to give the excitability a better chance of being renewed, in case it was possible, after the division of the nerves. The larger frog was then taken, the nerves supplying his leg were divided, and a portion removed just before they enter the ham, (this is necessary on account of a branch which enters the upper part of the gastrocnemius muscle,) a circular incision was then made through the skin at the ankle, which was then slipped up over the muscles as high as the knee.

leaving the muscles bare; salt was then sprinkled upon them, when irregular contractions were excited during twelve minutes. The salt was then carefully washed off, the skin was replaced, and the frog was put into the water again. The smaller frog was now treated similarly, except that the nerves were not divided. The salt excited contractions in these muscles during fifteen-minutes. The salt was then washed off, and this frog was replaced in the water with the other. The muscles of both had entirely lost their power of contraction. They were allowed to remain five weeks in the water. At the end of this time the whole of the foot of the large frog, whose nerves had been divided, was found to have sloughed off, and upon again slipping up the skin, and reapplying salt to the muscles, not the slightest contraction or even fibrillary motion could be perceived in them—they were a good deal injected with blood, were softer in their texture than natural, and there was some interstitial deposition of fluid in them, thus evidently shewing that the secernent as well as the motor function had suffered. Upon examining the nerves they were found to have been completely divided. The muscles of the smaller frog, whose nerves were entire, contracted as readily as at the first application of the salt, though they were exhausted in a much

shorter time, their colour was rather darker than that of the healthy muscles, but their texture appeared unchanged, and there was no interstitial deposition of fluid in them.

This experiment, which may be considered as the completion of Dr. Philip's, proves directly the reverse of the position he has endeavoured to establish; because the power of those muscles whose nerves were divided was not restored during rest. "The general powers of the constitution," which by the bye is so vague an expression that one hardly knows what it means, are not sufficient to restore the excitability, because the frog was particularly vigorous and healthy; but it was restored in those muscles whose nerves were entire. Here then we have evidence bearing directly upon the true point, proving that the excitability of voluntary muscles after being exhausted cannot be restored when their communication with the brain is cut off. Excitability therefore is entirely dependent on the nervous system, and there can be no longer any doubt of Dr. Philip's having failed to prove the independent nature of the muscular power, because excitability and that power or influence which the voluntary muscles receive from the cerebrum through the medium of the anterior, or nerves of the second class, by which they are enabled to contract, are

clearly one and the same. As the terms Irritability and Excitability have hitherto been made use of to express a power which has no existence, if they were retained, it would be some time before the idea conveyed by them would be separated from their original signification: I think it less objectionable to discard them altogether than to employ them in a new sense.

We are now able to explain why all power of contraction in the muscles is lost when death is occasioned by electricity, or by crushing the brain with the blow of a hammer, without having recourse to the curiously cautious expression of Dr. Philip, who thinks he removes all difficulty by saying "the nerves may be *influenced* through the brain and spinal marrow even to the entire destruction of their functions, in which case no sign of irritability is perceived after death." The fact is there are different ways of destroying the nervous system, either suddenly or slowly, but all alike effectual. Electricity pervades the whole system, the blow of a hammer in crushing the brain gives a severe shock to every nerve connected with it, consequently their functions cease suddenly. When the brain is only removed, no great degree of injury is committed upon the nerves; in this case therefore their functions cease gradually: but it is as absurd

to argue from that fact that the greater part of the vital functions are independent of the brain, as it would be to argue that a flower is independent of the stalk on which it grows, because it does not wither the moment it is gathered.

I consider these facts and observations as fully warranting my opinion that there is a something, which is neither chemical nor animal electricity, but a fluid *sui generis*, and which I think is appropriately expressed by the term Cerebral Influence, constantly and uninterruptedly flowing from the cerebrum along the anterior nerves, and this is the reason why every muscle which receives filaments belonging to the second class of nerves is subject to the will.

CHAPTER IV.

*On the FUNCTIONS of the THIRD ORDER
of NERVES.*

IN entering upon the examination of the experiments which Dr. Philip considers as establishing the fact that the action of the heart and vessels of circulation is independent of the nervous system, the reader should bear in mind the reasons already given in the second chapter why the passive state of the voluntary muscles, and the continuance of the action of the heart for a short time, and its subsequent insensibility to stimulants, proves nothing more than that the action of the former is, and that that of the latter is not, dependent on volition. There is a fact also which throws a great degree of suspicion upon all the experiments that have ever been brought forward to prove that muscular motion is independent of the nervous system; it is, that they all succeed best in animals newly born or quite young, at which time this system bears a much larger proportion to the rest of the body than at any future period of their existence; for this proportion diminishes gra-

dually as an animal approaches the attainment of its full growth.

It will be sufficient to select a few of the most decisive of those experiments which shew the effect of destroying suddenly, slowly, or wholly removing the brain and spinal marrow, upon the action of the heart and vessels of circulation, both in warm and cold blooded animals. If it shall appear that these experiments, instead of supporting Dr. Philip's opinion, prove that all involuntary motion is injured in proportion to the degree of injury committed upon the nervous system, I think it may fairly be concluded that no farther evidence is wanting to prove that this, as well as voluntary motion, is entirely dependent on an influence derived from the brain.

“ Exp. 19. The brain of a large frog was crushed by the blow of a hammer. The heart immediately performed a few quick and weak contractions. It then lay quite still for about half a minute. After this its beating returned, but it supported the circulation very imperfectly. In ten minutes its vigour was so far restored that it again supported the circulation with freedom, but with less force than before the destruction of the brain. An instrument was then introduced under the heart, and after ascertaining that this had produced no change

on its action, the spinal marrow was crushed by one blow, as the brain had been. The heart again beat quickly and feebly for a few seconds, and then remained still, and seemed wholly to have lost its power. In about half a minute it again began to beat, and in a few minutes acquired considerable power, and again supported the circulation. It beat more feebly, however, than before the spinal marrow was destroyed. It ceased to beat in about an hour and a half after the brain had been destroyed. In another frog, after the brain and spinal marrow had been wholly removed *without any farther injury being done to them*, the heart beat for nine hours, gradually becoming more languid.

“ Exp. 20. Two rabbits were instantly killed by crushing the brain with a hard substance. In both the heart immediately beat with an extremely feeble and fluttering motion.” No mention is made of how long or short a time this motion continued.

“ Exp. 23. A rabbit was instantly killed by crushing only the anterior part of the brain by a blow. The side was rendered hard by spasm for about half a minute. Neither during this, nor after it, could the author perceive any motion of the heart by applying the hand to the

side. The head was then cut off, about three quarters of a minute after the brain had been crushed. No blood spouted out and very little ran from the vessels

“ Exp. 32. The spine of a frog was laid open at the lower end, and the animal suddenly killed by a wire, of nearly the same dimensions with its cavity, passed through it as in M. Le Gallois’ experiments. The web of one of the hind legs was then brought before the microscope, and the circulation in it was found to have wholly ceased.

“ Exp. 24. A strong ligature was passed round the neck of another rabbit of the same age with that used in the preceding experiments. It was suddenly tightened, and the head cut off. In this instance *little spasm* took place, and the heart was found beating regularly under the finger for about three quarters of a minute. At the end of this time the ligature was slackened, and the blood spouted out to the distance of three feet, and continued to spout out with great force till nearly the whole of the blood was evacuated.

“ Exp. 12. A frog was immediately killed by destroying the brain and spinal marrow with a *small* wire. After it had lain dead for several minutes, part of the web of one of the

hind legs being brought before the microscope, the blood was seen circulating in it as rapidly as in the web of a healthy frog.

“ Exp. 5. A rabbit was rendered insensible by a blow on the occiput; artificial respiration was not performed. The spinal marrow was destroyed by introducing a small hot wire through an opening between the cervical and dorsal vertebræ, first through the upper portion into the brain, then through the under portion to the end of the spine. On laying open one side of the neck, the carotid artery was found beating: on dividing it, blood of a dark colour was thrown out copiously per saltum.

“ Exp. 51. The spinal marrow was wholly removed in a newly-dead rabbit, without at all affecting the motion of the stomach and intestines. The removal of the brain produced as little effect upon it as the removal of the spinal marrow.

“ Exp. 27. When a frog is decapitated without much loss of blood and then a ligature thrown round all the vessels attached to the heart, on the web of one of the hind legs being brought before the microscope the circulation in it is found to be vigorous, and will continue so for many minutes, at length gradually becoming more languid.

“ Exp. 66. A rabbit about two months old

was killed by a blow on the occiput. The chest was then laid open and a ligature was thrown round the aorta. Part of the mesentery was now brought before a microscope and the blood in its vessels was seen moving with great velocity. By examining different parts of it and choosing those which had not been previously disturbed, and consequently still retained some warmth, the circulation was found going on with rapidity in the smaller vessels for *a quarter of an hour after the aorta had been secured*, and an irregular motion of the blood in these vessels was evident for twenty minutes longer, the blood stopping and going on, and sometimes moving backwards and forwards in the same vessel. This could be distinctly seen long after the part had become quite cold. This experiment was performed in the sunshine, in the open air, where there happened to be a good deal of wind, and the exposed part of the mesentery quickly became parched; which as we found from other trials destroyed the motion of the blood in the capillaries long before it naturally ceases.

“Exp. 67. A dead rabbit about a month old, whose intestines had been submitted to examination after a ligature had been thrown round all the vessels attached to the heart and this

organ removed, was thrown aside. *An hour and a quarter after the heart had been removed,* the author brought part of the mesentery, which had long been quite cold, before the microscope, and still found the blood in some of the capillary vessels moving freely."

In these experiments the reader will have perceived that the greatest injury committed upon the nervous system is that which is caused by suddenly crushing the brain and spinal marrow. It is also evident that by destroying the brain in this manner, a most severe shock is communicated to every nerve connected with it and the spinal marrow. In the frog the heart ceased to beat *seven hours and a half sooner* than when the brain was simply removed. In the warm blooded animal the heart beat with a feeble fluttering motion for a *few seconds*. In the frog whose spinal marrow was suddenly crushed the action of the vessels of the web ceased *immediately*.

It is also evident that when the brain and spinal marrow are either slowly destroyed, or carefully removed, no such shock is communicated to the rest of the nervous system, and the injury is more immediately confined to the parts implicated in the experiment; consequently the circulation continued for a short time in the web of the frog, and the carotid

artery of the rabbit spouted forth blood per saltum. That the injury was in a great measure confined to the brain and spinal marrow in these experiments is proved by its being mentioned that "very little spasm took place."

It is also no less evident that in Exp. 66 and 67, where the heart was simply cut out and the brain and spinal marrow left untouched, that the motion of the blood in the minute vessels was least affected, for they continued acting a considerable length of time.

These are some of the most decisive experiments which Dr. Philip has brought forward, but where in them do we find unequivocal evidence of motion after the entire destruction of the nervous system? Is it to be found in the cessation of all motion upon the entire destruction of that system? In its continuance for a short time after its *partial* destruction? or in its continuance for a considerable length of time after death, where the nervous system had received the smallest amount of injury? Do they not rather prove that all motion is affected in proportion to the injury which is committed on that system, and consequently that involuntary motion is as entirely dependent upon it, as all voluntary motion has already been proved to be? They also prove that the nerves themselves have undoubtedly an action to a certain extent independent of the brain

and spinal marrow, precisely as the minute vessels of circulation, as shewn in Exp. 66 and 67, have an action to a somewhat similar extent independent of the heart. These opinions are strongly corroborated by the observations of M. Flourens and of Dr. Marshall Hall, who have taken notice of the gradual cessation of the circulation which follows the careful removal of the brain, commencing in the most minute extremities of the blood vessels, where, provided the brain be the original source from whence the nerves derive their influence or power, it is in the failure of the action of these minute vessels, on account of their being most distant from the brain, that we should naturally expect the effect of its removal would be first perceptible.

In thus ascending from effects to causes, it is found that the brain is the source whence the influence of the nerves is derived, and that these again have the power of distributing it for a short time after the removal of the brain so long as any influence remains in them, precisely as they did before the connexion was interrupted: but it must not be forgotten that the moment at which the brain is either removed or destroyed, is exactly the time at which the failure of all motion commences. These facts and observations lead to the infer-

ence that the same relation subsists between the brain and nerves, as between the heart and arteries; the brain being the source whence the nerves derive their influence, the heart the source whence the arteries derive their supply of blood.

The reader has now seen that none of these experiments afford evidence of the contraction of a single fibre after the destruction of the nerves, consequently, they do not in the least support the doctrine which teaches that Irritability or Excitability is a property belonging to the fibres themselves, and dependent on their mechanism. The rule, therefore, as laid down by Sir Isaac Newton, whom no one can accuse of having arrived at hasty conclusions from uncertain premises, "that sound philosophy teaches that activity in matter can never be inherent," must be restored to its place among established truths.

Those experiments which Dr. Philip instituted to shew that the action of the heart and vessels of circulation may be influenced by stimulants and sedatives applied to the brain and spinal marrow, prove the fact sufficiently well. The part being laid bare, and the spirit of wine or a solution of tobacco being applied, the motion of the heart is accelerated or retarded. This effect is always produced pro-

vided the stimulant is applied to a large extent of surface. An intense stimulant applied to a small part produced no effect whatever upon the heart. Dr. Philip also observed, that the effect of the sedative was not so immediately apparent as that of the stimulant.

There can be no doubt of these effects constantly taking place so long as the communication of the nerves with the brain remains entire. There is, however, very great reason to believe that Dr. Philip has been deceived in the result obtained from his seventeenth experiment; for upon repeating it, I could not perceive that the heart could be influenced by spirit applied to the brain after the division of the spinal marrow, provided care was taken to prevent the spirit from affecting that part of the spinal marrow which the incision had separated from the brain. Unless this is avoided, the effect will of course be the same as if the spinal marrow had not been divided at all; but when it was properly managed, the contractions of the heart became gradually both fewer in number per minute, and weaker in degree. This was repeated several times on frogs, but always with the same result, the action of the heart was never increased a single beat per minute, though the spirit was kept in contact with the brain. The only way in which I could succeed

in preventing the spirit from touching that part of the spinal marrow which was separated from the brain was by dividing the spine just below the brain with the blade of a small knife and leaving it in the wound, then, and only then could I be certain that the spirit was applied to no other part of the nervous system than the brain alone. There is the more reason to believe that Dr. Philip has been deceived in this as well as in his 26th Experiment, in which he thinks he has proved the same thing in a rabbit, because if he can prove that a stimulant applied to the brain can affect the heart after all nervous communication between them is cut off, he ought also to be able to shew that a stimulant when applied to the brain after it has been altogether removed from the body, will affect the heart as well as when it was in the skull.

The time is now arrived when I may venture to assert without fear of contradiction, that of the two facts respecting which Dr. Philip says, "of this apparent inconsistency M. Le Gallois justly remarks, that two facts well ascertained, however inconsistent they may appear, do not overturn each other, but only prove the imperfection of our knowledge," only one has been well ascertained, and that one is the least important of the two, for surely no experiment

was necessary to prove that the action of the heart was influenced by emotions of the mind. In making this statement, Dr. Philip and M. Le Gallois are precisely in the situation of a person who knows that by moving the regulator of a watch he is able to influence the motion of the hands, yet is entirely ignorant of the means by which the effect is produced, and who, upon observing that the second, minute, and hour hands, had a motion independent of each other, would conclude that they could not be dependent upon one spring, simply because he knew not the mechanism. Since, therefore, one of the facts was not well ascertained, there can be no inconsistency, because if the brain is in constant action to maintain the motion of the heart, every variation in the action of the former must be followed by a correspondent variation in that of the latter, as a necessary consequence. The terms stimulant and sedative are used to express nothing more than increase and diminution. At the time of the commencement of these experiments the heart was beating, and the application of either the one or the other to the brain or spinal marrow excited no new action either in the brain or in the heart, but simply caused an increase or diminution, or in other words, a variation of that which already existed. As the increased or diminished

action of the heart was the result of a corresponding action of the brain caused by the application of either agent, so the ordinary action of the heart is necessarily caused by the ordinary action of the brain, and as the former invariably ceases to beat soon after the removal of the latter, it must be concluded, that so long as the heart is acting in the perfect animal, the brain is acting also.

In speaking of the results obtained by the application of different substances to the brain, Dr. Alison observes, "It is generally agreed among physiologists, and I think it cannot be doubted, that some *physical change* is produced in the nervous system by every mental act, which, through its intervention, influences any bodily function. If this be so, it may naturally be expected that these changes in the nervous system may be imitated by the application of physical agents, which furnishes a ready explanation of such agents applied to the nervous system on the various functions, and therefore when any effect is observed from the application of physical agents to the nervous system, analogous to the changes produced in the same function by mental acts or affections, instead of arguing from that fact the necessary dependence of the function in question, on any influence derived from the nervous system, it seems

more reasonable merely to suspect that those physical agents must have wrought a change on the nervous system, somewhat analogous to that which these mental acts or affections had been wont to produce.”* Every mental act, however, does not cause a perceptible change in the heart’s action, because this organ is never affected by ordinary efforts of the mind, as the exercise of the intellectual faculties, but only by those passions or emotions of the mind which absorb the whole or nearly the whole attention of the individual, such as fear and rage. Nor is it to be expected that all persons will be equally affected under similar circumstances, because that which is a cause of fear to a timid person may excite no such emotion in one courageous and habituated to danger, so that the effects of mental emotions upon the action of the heart will vary in almost every individual, according to his natural disposition. These considerations offer a ready explanation of the fact mentioned by Dr. Philip, namely, “that chemical stimulants applied to the brain are better calculated to affect the heart than mechanical stimulants, whereas the latter are better calculated to excite the volun-

* Quarterly Journal of Science, Literature, and the Arts, vol. ix. page 110.

tary muscles." A chemical stimulant, as he observes, must be applied to a large surface of the brain to produce its effect upon the heart, thus in a great measure imitating the mental emotions which occupy the whole attention of the individual; whereas a mechanical stimulant, in affecting only that small part of the brain with which it comes in contact, must operate very much in the same manner as the will must act in commanding the action of any particular set of muscles. Unless the will has the power of acting separately and distinctly upon every nerve, or rather perhaps upon their prime origins in the brain, it could not possibly have that power over the muscles which we know it does possess, but whenever we might wish to move a hand, we could never be certain that both hands would not be moved, or both legs, or legs and arms at the same time, and we should be wholly unable to calculate upon what motion we should next perform. We have no reason therefore to believe, that upon the application of any substance, whether stimulant or sedative, chemical or mechanical, any action takes place different from what has already many times occurred. When a stimulus is applied to a muscle it contracts, but this is no new action, nor in any respect different from what had taken place a thousand times

before, so also when an animal has received a blow on the occiput, and is opened with the least possible injury of structure, involuntary motion is found going on; crush the brain, and the heart beats no more; take away the brain, and the heart languishes from that moment; if, instead of removing it, spirit of wine be applied, the motion is accelerated; if, on the contrary, a sedative, it is diminished; but either quick or slow no new motion has been excited, nor any thing more than a variation of that which already existed and had occurred before. These are the considerations from which the dependence of the action of the heart and vessels of circulation upon the nervous system is argued, and by which we are enabled readily to comprehend why every variation of the action of the brain must necessarily be followed by a corresponding variation in that of the heart.

Dr. Philip thinks it quite sufficient that the organs of involuntary motion should be supplied with nerves, in order that they may be influenced through the nervous system, so that the power by which secretions are formed, and the supply of fluids from which they are formed, may be always maintained as nearly as possible equal to each other. It may here be observed, that if the nerves of the heart were of no farther use to the motion of that

organ than to subject it occasionally to the influence of the brain, when this organ is itself affected, we can scarcely believe it would receive so large a supply of nerves as it is known to have, a very few filaments would be sufficient for this purpose, so that according to Dr. Philip's views, the supply is still much larger than there is any necessity for, and some might be dispensed with. In answer to this objection, it may perhaps be urged that no organ is altogether dependent upon a single vessel for its supply of blood, therefore it is not to be expected that an important organ like the heart would be provided with nerves from a single source. This answer appears at first to have some weight, but I think it is entirely set aside by considering, that although the circulation of the blood may in a short time after the division of the main artery of a limb be carried on perfectly well by means of anastomosing vessels, the limb does not recover its healthy state and vigour until this is fully accomplished; we have no reason therefore to suppose that any part receives even more blood than is necessary. Again, the parallel can scarcely be said to be just, since the great proportion of facts obtained both by observation and experiment is decidedly against the idea of there being that communication among nerves which can enable

them to compensate for or supply the place of any which might be divided or paralysed. In explaining why the action of the heart should be occasionally increased, in order that more blood should be sent to secreting surfaces, at the time when the nervous influence is most active, Dr. Philip alludes to the increased secretions of the kidneys and bowels, which in many persons have been known to be caused by affections of the mind, and in instancing these occurrences as examples, he falls into one of those inconsistencies which are so frequently met with in the writings of those who argue from incorrect premises. In some experiments he found that when the supply of nervous influence to a secreting surface was diminished, but not altogether cut off, the secretion from it was not stopped, but increased in quantity and deteriorated in quality. At page 114, he says, "both in the stomach and in the lungs they were sufficiently copious. The fault seemed to be, that a due change on them had not been effected;" and at page 157, "it is evident that affections of the nervous system could produce no occasional increase of the secretions, were not the sanguiferous system, and particularly the vessels of secretion, capable of being stimulated by the same influence which operates in the formation of the secreted

fluids. The increase of secreting power would be in vain, were there not at the same time a corresponding increase in the supply of the fluids on which it operates." The phenomena of increased secretions are observed in those who are under the influence of depressing passions, of which fear is perhaps the most common. The effect of these emotions is, as their name implies, to depress or to diminish the action of the brain according to their intensity; some instances have indeed occurred, where the action of the brain has been thus suspended altogether, and the individuals have dropped down dead. But though such extreme instances are rare, the minor effects of the depressing passions are of very frequent occurrence, among which is to be reckoned that of fainting. When a person is under the influence of extreme fear, and the action of his brain thus diminished, that of his heart and vessels of circulation is always feeble, as is proved by the universal paleness and coldness of the body, so that when, according to Dr. Philip, the flow of blood and of nervous influence to secreting surfaces should be greater to account for the increased product, there is every reason to believe it to be actually less.

But while the observations of Dr. Philip are at variance with his experiments, it is extremely

interesting to remark how admirably the latter coincide with the phenomena above mentioned, for it is when the supply of nervous influence to secreting surfaces is diminished, by reason of the operation of depressing passions upon the brain, that the secretions are both increased in quantity and deteriorated in quality, as is evidenced by the pale limpid character of the urine, and the loose consistence of the evacuation from the bowels, which is passed on such occasions. There can, I think, be but little doubt that this is also the case with the profuse cold perspiration which bedews the skin at the same time. The only difference between the experiment and the natural occurrence is, that in the one case the effect is only observable in the parts which were supplied by the divided nerves, in the other it is universal, and affects all parts alike.

The increased frequency of the pulsations of the heart of a person under the influence of fear, may perhaps by some be mistaken for an increase of power, and therefore be considered as arguing against what has just been stated, but this is nothing more than the natural result of its vigour being impaired, because if the heart beats feebly, it will not contract with sufficient force to throw the whole of the blood which it contains into the pulmonary arteries and aorta,

under these circumstances, the state of diastole must necessarily be of shorter duration, because the rush of blood from the veins continuing the same, the ventricles will be filled again in a less time than if they had been completely emptied by a vigorous contraction; the contractions of the heart, therefore, will be more frequent in proportion as they are feeble. The rapid, feeble pulse of a person in the latter stage of an acute attack of fever, when all his vital powers are ebbing fast, is the "*testis mearum sententiarum*," and is not to be compared with the full bounding pulse of a person under the influence of the exciting passions. It is worthy of remark, as contrasted with the opinions of Dr. Philip, that in such a person, in whom, from the redness of the surface of the body, and the general evidence of increased activity of the circulation, there is great reason to believe that secreting surfaces receive a proportionate increase in the flow of blood, no particular increase of secretion is perceived; not but what perhaps it may take place in some degree, though neither in any great quantity, nor with rapidity enough to excite any action necessary for its expulsion. It is more than probable that the effect of an increased supply of nervous influence and of blood to secreting organs, would only be that of rendering their products per-

fectly healthy, in which case the eye of the observer would detect no alteration in them; but it must be remembered, that as yet nothing has been done by physiologists in their experiments beyond *destroying and lessening it* by dividing and otherwise injuring the nerves.

I consider the preceding facts and observations as fully justifying the conclusion, that the action of the heart, and in fact of all parts supplied by the third order of nerves, or those which are composed of sympathetic and posterior filaments, is entirely and constantly dependent upon the brain and nervous system; and farther, that as involuntary motion does not cease immediately upon the removal either of the brain or spinal marrow, the reason why this motion is independent of volition is not to be sought for in either of these organs, but in the nerves themselves, and will be found to depend upon the manner in which the influence of the brain having been received by the nerves, is again by them supplied to the sympathetic ganglions.

SECTION I.

*On the Dependence of the SECERNENT FUNCTION
on the NERVOUS SYSTEM.*

THE question, whether the power by which muscles are enabled to contract is dependent upon the nervous system, has as yet principally engrossed our attention: but besides a capability of contraction, and a means whereby they may be caused to act when necessary, it is no less requisite that their structure be maintained in a healthy state, so that they may be fit to act. This is the object of what has been termed the secernent and excernent function, which comprehends, besides the formation of particular secretions and excretions, the growth and nutrition, the deposition of new and the removal of old parts throughout the whole body. It will not be necessary to consider the functions of secretion and excretion separately, because whether a substance is deposited in one part of the body for the purpose of forming one of its component parts; or whether in another to be carried off as no longer useful, both must be accomplished by a similar action of the vessels, and must be equally under the control

of one power, whatever that may be. The next question therefore to be considered is, whether the secernent function is maintained and performed by the nervous system—upon this point there is no lack of evidence, as far as relates to the simple fact, for who has not divided the 8th pair of nerves? My object however is to go farther into this question, and to ascertain if possible the mode in which secretion is effected. As the blood is evidently the fluid from which all secretions are formed, the first step in this inquiry is to ascertain the cause of its fluidity. There must be some reason for its being fluid while circulating, because when it is removed from the living body it always has a tendency to coagulate, and to become unfit for motion. But before this can be properly entered into, there is another question which ought to be disposed of, namely, whether nervous influence and galvanism are identical? The simple inference from the experiments of dividing the 8th pair of nerves is quite clear, because there can be no doubt of their division being the cause of the failure of the secreting power of the stomach; but when Dr. Philip stimulates the divided nerves by means of galvanism, and finds that some degree of digestion may thus be continued for a short time, and therefore concludes nervous influence and gal-

vanism to be identical, he draws a farther inference, which may or may not be correct.

The reader will learn from the following passage, which occurs in a reply of Dr. Philip's to some objections to his opinions which were raised by Dr. Alison, how fully he thinks the identity established. "Those who have hitherto objected to my conclusion from the experiments in question, either maintain that we must rather suppose the existence of a power capable of the most complicated functions of the nervous influence, yet distinct from it, than admit the identity of these powers; and that, without attempting to shew, that in any of their properties they are incompatible with each other; an instance of more erroneous reasoning than which it is impossible to adduce; or that galvanism excites the nerves of the part to prepare nervous influence and thus perform the office of the brain or spinal marrow, of which every fact relating to their functions proves them incapable. We might as well, I conceive, suppose a bone as a nerve preparing this influence."*

Exp. 77 is one of those upon which Dr. Philip relies for the proof of the identity of the

* Quarterly Journal of Science, Literature and the Arts, Vol. ix. p. 261.

two powers; in it therefore we should expect to find certain proof that galvanism restores what is lost by the parts being separated from their communication with the brain. The details of this experiment are as follows:—"Two small dogs, of the same size and age, were kept without food for about thirteen hours, they were then permitted to eat as much raw mutton as they chose. In both, the 8th pair of nerves were divided immediately after they had taken the mutton. In one of them the nerves were coated with tin-foil, as they had been in the rabbits, a three-shilling piece having been previously bound on the pit of the stomach and lower part of the thorax, after the hair had been shaved off; and galvanism applied as in the foregoing experiments.

"The apparatus had been so arranged, that the galvanism was applied as soon as the nerves were divided.

"The dog which was not galvanised was almost immediately affected with dyspnæa, and within ten minutes with repeated efforts to vomit. The other, to which the galvanism was applied of sufficient strength to occasion a very gentle motion in the fore-legs, but not any expression of pain, breathed as free as before the division of the nerves, and never made any effort to vomit. The application of the

galvanism was twice discontinued for a few seconds, during which the animal breathed very laboriously, but on renewing the galvanism the breathing immediately became free. This dog lived *two hours and a quarter*.

“On opening the stomach after death, we found the mutton half digested. It had lost its red colour, and was reduced to a soft pulpy substance, in which there was little or no appearance of muscular fibre. That part of the mutton which lay in the pyloric end of the stomach was most digested, a proof that digestion was going on in the usual way. The vessels in some parts of the stomach, and throughout the whole of the small intestines, were highly injected, giving those parts a very florid appearance. The lungs were rather redder than natural, but otherwise quite healthy, collapsing perfectly on the thorax being opened.

“It is of great consequence, in judging of the effects of galvanism on the lungs, that the galvanic apparatus should be arranged before the commencement of the experiment, that the animal may be subjected to its influence as soon as the nerves are divided, and thus the difficulty of breathing wholly prevented. The other dog, which was *still alive at the end of four hours after the nerves had been divided*, was killed at this time. The mutton, although it

had been in its stomach so much longer than in the other dog, was as firm as when it was swallowed, and perfectly retained both its red colour and fibrous appearance, except that on the outside the bits seemed as if they had been dipped in boiling water; immediately below the surface they were quite red. The lungs exhibited the same appearances as those of rabbits under the same circumstances. They were so obstructed that they collapsed very imperfectly, and their surface was covered with patches of a dark red colour.

“There was nothing in the stomach of either dog but the mutton, which was taken at the commencement of the experiment, and no part of it had been thrown back into the passage leading to the stomach in either.”

In Exp. 75 two rabbits were employed. When the galvanised rabbit died, “*the other rabbit, which had been left undisturbed, was killed at the same time.*”

The average length of time which a rabbit lives when galvanised, after the division of the eighth pair of nerves, appears to be about eleven hours; those who are not galvanised seldom die under twenty hours. This fact, of which Dr. Philip has not taken the slightest notice, is of itself a complete bar to the idea of nervous influence being identical with galvanism. If

however we simply consider what galvanism is, its incompatibility with nervous influence will be soon apparent. Galvanism, when applied to the nerve of a muscle of voluntary motion, excites contractions in that muscle; therefore it acts as any other stimulant. Every fact with which we are acquainted proves that a certain quantity of nervous influence remains in the nerves of secreting organs after their division, as well as in those belonging to muscles. Now galvanism is a stimulant, which is capable of pervading all parts to which a nerve is distributed more completely than any other stimulant that can be employed; consequently a steady and well-directed application of such an agent will stimulate the nerves of the stomach and lungs to continue the action upon the blood in the vessels of these organs, which they were performing at the time of their division, until they are exhausted. This view of the subject offers a ready explanation of the reason why galvanised animals invariably die sooner than those which are left undisturbed after the division of their nerves, but which is otherwise utterly incomprehensible. However much this explanation may be at variance with the opinion of Dr. Philip, it is not contradicted either by any single fact or any number of facts which he has brought forward; but on

the contrary all his experiments are much better explained by supposing galvanism to be nothing more than a stimulant capable of pervading all parts.

In speaking of the application of this agent for the relief of asthma, he says it is necessary to move the negative pole of the battery in order to avoid the mortification which is occasioned by its being too long applied to the same spot. Here is another fact which is incompatible with the idea of the two powers being identical; but if it be allowed that galvanism is a stimulant which exhausts nervous influence as all others do, it is easily understood why the death of a part should result from the exhaustion of the power which maintained its life, by the too long continued application of the galvanism. Another fact, noticed by Dr. Wedemeyer, will be presently mentioned, which will place this subject in a still stronger light. If such fancies as these be admitted, where can we fix a limit to them? for there is as much reason to suppose spirit of wine to be nervous influence, because it increases the action of the heart when applied to the brain; or salt, because it excites contractions when applied to a voluntary muscle. Galvanism is a stimulant when applied to the nerve of a muscle, and there is no reason whatever to suppose

it otherwise when applied to the nerve of a secreting organ : its effect in relieving certain cases of asthma is clearly referrible to this cause.

Dr. Alison has taken up this question in the volume of the Quarterly Journal of Science, Literature and the Arts, already mentioned, in a somewhat extraordinary manner, and thinks if it can be proved that the non-identity of these two powers can be proved, *it follows* that the secernent function is altogether independent of the nervous system. There are but few, I believe, besides Dr. Philip himself, who doubt his having succeeded in the first part of his position ; but when he says, “galvanism can only be explained by considering it identical with nervous influence,” and “if we can make it probable that the changes which occur in the nervous system are not galvanic actions, we need go no farther after these experiments in order to shew that the nervous system is not necessary to secretion”—it is in direct opposition to facts which have hitherto been regarded by physiologists as well established. Dr. Alison endeavours to explain the failure of secretion resulting from the division of a nerve by supposing a “noxious influence” to be conveyed by the injury to the part where the process of secretion is performed. At page 112 he says,

“ by dividing the eighth pair of nerves after an animal has taken food, the natural sensations which accompany and succeed the reception of food into the stomach, are prevented from taking place, and the painful sensations of dyspnæa and nausea are produced; and may not this be sufficient to explain the failure of secretion in the stomach?” Dr. Alison seems here to have forgotten that it is as impossible for a painful sensation to arrive at the brain from the lungs and stomach, as it is for a natural sensation, after the division of these nerves, since from that moment the brain must necessarily lose all consciousness of the existence of these organs. This observation is also evidently made with a total disregard of the comparative effects upon the secretion of the stomach, resulting from the division of the spinal marrow at different parts, as shewn by Dr. Philip, and which are in point of fact the most important of all his experiments. When this organ was destroyed from the dorsal region, the injury to the secreting power of the stomach was *greater* than when the lumbar portion was destroyed; thus proving the effect upon secretion not to be in proportion to the intensity of the injury, for in that case the result of both experiments would have been the same, but commensurate with the extent

of the injury committed upon the nervous system. Again, at page 117 he says, "In order to account for solids and fluids, we must either suppose the galvanism transmitted by the nerves to the various parts of the body to be different in its nature in different places; or we must suppose the blood to undergo very various preparation before it is submitted to the action of the galvanism." There would indeed be room for wonder and astonishment if we were to find a liver formed of muscular structure secreting bile, and a kidney formed of similar structure secreting urine; then there might be some necessity for supposing a particular sort of nervous influence or of blood to be sent to each part; but if we simply suppose the blood and the nervous influence to be everywhere alike, and the difference in the appearance and character of the secretions to be the natural result of the difference of the structure and arrangement of the vessels in each secreting organ, every difficulty vanishes, and we need feel no more astonishment than we do on being told that cotton thread upon being put into one piece of machinery will reappear as a stocking, and from another as a piece of calico, though our admiration of the Almighty power will be increased at finding such various effects produced by such simple means.

At page 145 of the "Outlines of Physiology," Dr. Alison again endeavours to support his supposition of a noxious influence being conveyed by the injury of a nerve, and says, "It appears from experiments of Le Gallois, Philip, and especially Flourens, that great mechanical injury of the brain or spinal chord weakens the circulation in the capillaries sooner and in a greater degree, than that in the large arteries, and even that such injury of the spinal chord is often seen to affect immediately the flow of blood in the capillaries of those parts only which have their nerves from the injured portion of the spinal chord." This is explained by Dr. Alison in the following manner:—"Those mechanical injuries of the human body which cause insensibility by general concussion of the system, at the same time manifestly and sometimes irretrievably depress the circulation; which effect it is reasonable to refer to an impression on the nervous system, because of the accompanying insensibility (which, as we shall see, implies affection of that system), and because it is just similar to the effect produced without much general concussion of the body. In some such cases of concussion of the human body, as well as in such experiments, violent convulsions attend the insensibility and the failure of the heart's action; the same impres-

sion on the nervous system, which excites the voluntary muscles, acting as a sedative on the vital power of the heart.

“The inordinate action of the bladder and consequent incontinence of urine, which gradually ensue in many cases, after injury or disease of the spinal chord, may also perhaps be regarded as an example of increased vital power in a muscular part from physical irritation of nervous matter.”

The results of these experiments referred to by Dr. Alison are fully explained by what has been already advanced respecting voluntary and involuntary motion; but how the incontinence par égorgement resulting from disease or injury of the spinal chord can be adduced as an instance of increased vital power in the bladder, I am at a loss to comprehend. It is however with physiology as it is with religious matters, the moment that men attempt to explain that which the limited extent of their knowledge does not allow them to understand, a door is opened to the wildest heresies.

In having left unnoticed the comparative results of those experiments of Dr. Philip, (division of spinal marrow,) which I have just alluded to, Dr. Alison leaves some of the most important proofs of the agency of the nerves being necessary to secretion quite untouched and unaffected by any of his observations; and in

having omitted to bring forward any facts to shew cause why his idea of a noxious influence being conveyed to a secreting organ by the division of its nerves, should be received in preference to that which has so many facts in its favour, it must of course remain as a mere supposition and be valued accordingly, because we have the authority of Sir Isaac Newton for saying that "conclusions drawn from experiments and observations by induction are not to be shaken by any objections, but such as are drawn from experiments or other certain truths."

One of the most decisive proofs of the agency of the nervous system being necessary to secretion, is to be found in the fact that in very young animals, in whom it is necessary that the secretory function, upon which the growth and nutrition of the whole body depends, should be rapidly and actively carried on, the brain and nervous system bear a much greater proportion to the rest of the body than at any other period of their existence, and which proportion is observed to diminish gradually as an animal approaches its adult state, when of course a power requisite for any farther increase of growth is no longer necessary, but only such as may be sufficient to maintain it in a state of maturity and health. Does not this fully explain the fact, that children cannot bear opium

or any medicine which tends to diminish the energy of the nervous system, as well as the reason why, upon any irritation of the bowels, they are so readily affected with convulsions, which, as has been already shewn, implies debility of that system? on the other hand they bear large doses of calomel, because it is rapidly carried off by the activity of the secretory function.

Lest any stress should be laid upon an experiment performed by Mr. Mayo,* in which, after the division of the nerves of the renal plexus of a dog, and the inclosure of the ureter in a ligature, some urine was found in the upper part of the ureter when the animal was killed, as containing direct proof against what has just been advanced; it should be observed that in order to have obtained a satisfactory result, that gentleman ought to have allowed some time to elapse between dividing the nerves and tying the ureter.

By this mode of proceeding he would have allowed the urine, which was being formed in the kidney at the commencement of the experiment, to drain off. If this had been done and urine had then been found in the ureter above the ligature, there would have been some grounds for concluding that the effect of the division of the nerves of the kidney was different from that of every other nerve which has as yet been experimented upon.

* See his work on Physiology, p. 121.

SECTION II.

On the CAUSE of the FLUIDITY of the BLOOD.

INDEPENDENTLY of the necessity of there being means by which the blood may be moved, it is no less necessary that it be maintained in a fluid state, and capable of being moved. The coagulation of the blood, which is observed to take place soon after its removal from the body, proves that its fluidity depends not upon any property inherent in itself; nor does it depend upon its motion in the vessels, because agitation does not retard this process when removed from them; but upon something which it derives from its contact with living parts, and which Mr. Hunter concluded was its life. The analogy noticed by him between the coagulation of the blood, and the stiffening of the muscles after death, and the well known fact, that any cause of death, acting in such a way as to produce this effect by means of a strong impression on the nervous system, at the same time that it prevents the stiffening of the muscles, prevents also the coagulation of the blood, renders it rather more than probable, that the cause of the life and motion of the one, is also the cause of the fluidity of the other. The cause of the former is re-

garded to be well established as dependent upon an influence derived from the nerves; while some late experiments by Dr. Wedemeyer, in his valuable researches on the powers which move the blood, appear to be inexplicable, unless we suppose the cause of the latter to be equally dependent upon the same influence. These experiments are also so conclusive as to the non-identity of galvanism with nervous influence, that I need make no apology for making use of the author's own words. He says, "When the lesser arteries were touched with either the negative or positive wire of a battery of from 14 to 24 plates, excited by a solution of salt, in a space of time varying from 10 to 30 seconds, nay, sometimes immediatly after the completion of the galvanic circle, a distinct contraction took place, which amounted to one-fourth, one-half, or even three-fourths of their calibre, the blood flowed more rapidly through the contracted parts. Nearer the heart the arteries were dilated, and the flow of blood was retarded. This contraction sometimes lasted a considerable time, nay, occasionally for several hours until the end of the experiment; in other instances, the contraction ceased in ten minutes, and the arteries resumed their former diameter. *A second application of galvanism to such arteries failed in general to cause any material contraction.* When this experiment was repeated on the

veins, a slow and trivial contraction appeared to me to be produced in one instance; but in every other the application of the wire was rather succeeded by gradual dilatation and retardation, or stoppage of the blood. On the capillaries the effect of galvanism was more striking; constantly there was produced more or less quickly, and for the most part within a minute, a remarkable retardation of the current of blood, amounting in no long time to complete stoppage and coagulation. The capillaries appeared enlarged, and if previously transparent and colourless, they now became bright red and as it were inflamed."

"This striking appearance of the coagulation of the blood and stoppage of the circulation in the capillaries on the application of galvanism is worthy of notice on several grounds. It cannot be the effect of the chemical action of one or other galvanic pole on the blood, as it is indifferently produced by either; neither is it the effect of contraction of the capillaries, for they do not possess the power of contraction in their parietes, at least I have never been able to develope it by the strongest chemical or mechanical stimulants, or by galvanism. I conceive that it arises from the influence of galvanism on the nerves and *through them* on the blood. When removed

from the influence of the nerves by its being withdrawn from the body, or by destruction of the nervous energy of the part, through violent inflammation or other excessive irritation, it coagulates. For its fluidity therefore is required a *certain natural state of nervous impression*: when this is disturbed the blood loses its fluidity. Galvanism then acts by disturbing, enfeebling, paralysing the function of the nerves which supply the capillaries, a conjecture which derives additional probability from the fact, that the small vessels, in which alone it causes coagulation of the blood, are much more liberally supplied with nerves than the larger arteries.”*

If the correctness of these experiments be admitted, it seems scarcely possible to arrive at any other conclusion as to the cause of the fluidity of the blood than that of Dr. Wedemeyer, and if the galvanism, like every other stimulant, exhausts the nervous influence, we are at no loss to understand why its reapplication failed in general to produce the same effect as the first. A strong confirmation of this opinion is also contained in the fact that in all those experiments upon rabbits adduced by Dr. W. Philip, those who were galvanised died several hours before those who were left un-

* Edinburgh Medical and Surgical Journal, No. C. p. 92.

touched; as these animals were to be the subjects of comparative trials, they were carefully selected as to size and age, therefore the galvanism is the only cause to which we can attribute the difference in the length of time which they lived after the division of their nerves. It is no objection that the coagulation of the blood was not observed in these experiments, as in those of Dr. Wedemeyer, because, in the first place, the microscope was not had recourse to in them, and in the next if it had been, the effect of the galvanism on so small an extent as the field of a microscope would be much more intense and easily observed than when diffused over the surface of the stomach, consequently, while the stronger stimulant exhausted, enfeebled, and paralysed the nerves, and thus caused the blood to coagulate, the slighter stimulant would only excite them to continue their functions till towards the end of the experiment, when it would be difficult to say whether the blood had coagulated sooner in the galvanised part than in the rest of the body.

SECTION III.

On the MECHANISM of SECRETION.

THE next points to be inquired into are, first, in what parts secretions are formed; and secondly, by what nerves, whether exclusively by those of the third order, or whether the second and fourth orders equally perform their part in this function.

There are some excellent observations bearing upon the first question, contained in the paper, "on the powers which move the blood," which has been already referred to. At page 84 it is stated—"The inner coat of an artery diminishes in strength and thickness along with the fibrous coat. At length both gradually terminate altogether in membraneless canals formed in the substance of the tissues." "The blood," continues Dr. Wedemeyer, "in the finest capillaries no longer flows within actual vessels, whose parietes are formed by a membranous substance distinguished from the adjoining cellular tissue by its texture and compactness, but in simple furrows or canals, whose walls are formed by the surrounding cellular tissue. This opinion, which he shares with several other late microscopical observers, is founded on the impossi-

bility of detecting with the microscope any membrane interposed between the parenchyma of the tissues and the blood moving in these extreme capillaries—on the facility with which single globules are seen, while passing through them, to quit the stream and penetrate among the ultimate globules that constitute the surrounding texture—on the rapidity with which the blood is observed to work out for itself a new passage or canal in the tissues, and on the impossibility of the processes of nutrition and absorption being carried on through the coats of vessels.” The above mentioned furrows or canals appear to be every where of the same diameter, by which arrangement it is evident that the exposition of the blood to the action of the nervous influence is rendered equal and uniform throughout the whole body.

As far as I am capable of judging, these observations of Dr. Wedemeyer appear decisive as to the locus in quo secretions are formed, while the fact, that small arteries receive an increased supply of nerves in proportion as their size diminishes, seems to point out, in a manner scarcely to be misunderstood, the power by which secretions are formed. It is extremely interesting to observe how beautifully this opinion coincides with that of Dr. Philip, who, although he does not appear to

have been well acquainted with the above mentioned arrangement of the vessels and nerves, has nevertheless proved by his experiments that “the vessels of secretion only convey the blood to be operated upon by the nervous power.”* Every thing therefore resulting from the action of the nervous influence upon the blood, ought to be considered as a secretion.

We now come to the question of whether this function is performed exclusively by the third order or sympathetic nerves. It is evident from the tenor of Dr. Philip's observations on the independent action of the small arteries and on secretion, that he considers the latter function to be performed wholly by the sympathetic nerves, not only in the viscera, but also in the limbs and general frame of the body, by means of the branches of those nerves which accompany the arteries in every direction. If it had been proved that the action of the vessels of circulation were independent of the nervous system, it would be impossible to attribute any other office to these nerves; but as Dr. Philip has failed to establish this point, it is as impossible to suppose that the small supply of sympathetic nerves which the arteries

* Exp. Inquiry, Chapter V. third edition.

receive can be more than sufficient to support their constant action. We now are able to understand how it has happened that Dr. Philip's adhesion to the doctrines of Irritability has caused him to neglect altogether to inquire whether secretion is performed by any other than the sympathetic, or third order of nerves; there is no experiment in Dr. Philip's *Experimental Inquiry* bearing on this point.

The wasting of the muscles, so frequently observable in paralytic limbs, affords a strong presumption in favour of the opinion that every nerve, whatever may be its class and order, (the purely cerebral nerves excepted,) presides over the secernent function of the part to which it is distributed. The emaciation of paralytic limbs cannot be altogether attributed to the want of exercise, because if a person thus affected remains in bed, the paralyzed parts shrink much more than those which are sound, from which it is fair to infer that the secernent function has been injured by the same cause which produced the loss of motion and sensation. There should not, however, be much stress laid upon such cases, because it is always extremely difficult (except in some cases of incised wounds) to ascertain what precise degree of injury or disease has caused the mischief, nor have we as yet any means of know-

ing what degree of injury will destroy the motor and sentient properties of nerves, without interfering with their secernent functions, yet such cases do evidently occur. The uncertain evidence which cases of this nature afford, extends but little beyond suggesting a probability, but this is the less to be regretted, since the labours of the ingenious, dexterous and indefatigable Majendie have made us fully acquainted with the extent to which the secernent function in each part is dependent upon its own nerves. The following are some of the important facts recorded by M. Majendie, as having constantly resulted from the division of the fifth pair of nerves within the skull:—
“Après vingt-quatre heures de la section, la cornée commence à devenir opaque; après soixante-douze heures, elle est beaucoup plus, l'opacité augmente, et cinq ou six jours après la section elle est de la blancheur de l'albâtre.

“Dès le deuxième jour, la conjonctive rougit, et sécrète une matière puriforme, lactescente, fort abondante; les paupières sont, ou largement ouvertes et immobiles, ou bien elles sont collées par les matières puriformes qui sont desséchées entre leurs bords; et quand on vient à les écarter, il s'écoule une assez grande quantité de la matière dont je viens de parler.

“ Vers le deuxième jour qui suit la section, on voit aussi l'iris devenir rouge, ses vaisseaux se développent, enfin l'organe s'enflamme. Il se forme à sa surface antérieure de fausses membranes, qui ont comme l'iris la forme d'un disque percé à son centre. Ces fausses membranes finissent par remplir la chambre antérieure de l'œil, et contribuent à faire paraître la cornée opaque. N'est ce pas un phénomène bien extraordinaire qu'une inflammation vive avec suppuration et insensibilité complète de la partie enflammée, et qui est causée par la section d'un nerf ?

“ Avant d'aller plus loin, je dirai que cette opacité rapide de la cornée me parut d'abord dépendre du contact prolongé de l'air. Pour m'en assurer, je coupai sur un lapin la septième paire de nerfs, qui, d'après les observations de M. Charles Bell, dirigent les mouvements de clignement : mais quoique l'œil ait resté sur cet animal en contact continu avec l'air pendant plusieurs jours, aucune opacité ne se montra sur la cornée, ni aucune inflammation, soit à la conjonctive, soit à l'iris.

“ Je vins alors à soupçonner que l'opacité dépendait du défaut de sécrétion des larmes. Il est possible me disais-je, qu'une membrane telle que la cornée ait besoin d'être continuelle-

ment imbibée par un liquide limpide pour conserver sa transparence. Pour m'assurer si ma conjecture avait quelque fondement, je fis sur deux lapins l'extraction complète de la glande lacrymale, mais aucune opacité ne se montra sur la cornée durant les huit jours qui suivirent cette extraction. Ma supposition n'était donc pas fondée.

“ Vers le huitième jour qui suit la section de la cinquième paire, la cornée s'altère visiblement; elle se détache de la sclerotique par sa circonférence, et son centre s'ulcère: au bout de deux ou trois jours, les humeurs de l'œil troublés et en partie opaques, s'écoulent, et l'œil se réduit à un petit tubercule qui n'occupe qu'une très petite partie de l'orbite; ce qui donne à l'aspect des animaux quelque chose de hideux.”

After some observations on the effect which this experiment has upon vision, M. Majendie continues: “ Quand un seul nerf est coupé, il se montre des altérations dans les narines, la bouche, la surface de la langue de ce côté; la moitié de la langue devient blanchâtre, son epiderme s'épaissit, les gencives quittent les dents, des matières alimentaires s'enfoncent dans les intervalles qui se forment; probablement que les animaux n'étant plus arrêtés par la sensation de la tendance des matières à

passer entre les dents et les gencives les y poussent sans s'en apercevoir."*

In these experiments the sympathetic nerves which accompany the arteries were not in the least injured; nevertheless, the process of disorganization commenced even while the blood was flowing through the vessels. If those nerves had maintained the nutrition and healthy condition of the eye, tongue, gums, the secretion of tears, &c. before the experiment, they would have continued to perform this office after it, and loss of sensibility would have been the only result obtained. It is clear, therefore, that the secernent function in all these parts was destroyed by the division of the fifth pair of nerves.

It appears to me that the only reason why the above-mentioned facts were unknown until published by M. Majendie, is simply because no one ever thought of performing the same experiment. In endeavouring to penetrate the mysteries of the secreting process, physiologists have been contented to multiply and reiterate experiments upon the pneumogastric nerves, until they believed that function could be performed by none other, although on account of their being so immediately essential to life, they are by no means favourable for the de-

* Journal de Physiologie Experim. et Pathol., tome iv. p. 178.

velopment of all the phenomena which result from the division of nerves. Even M. Majendie appears to have stumbled upon his discovery accidentally, while endeavouring to ascertain how far the fifth was instrumental in producing vision. As far as I know, Dr. Philip has the credit of being the first who, with a specific object in view, has ventured out of the old track, in determining what effect the division of the spinal marrow has upon the function of the stomach.

The fifth nerve, we have seen, is composed of posterior united to anterior filaments, and the muscles supplied by them are subject to volition. The pneumogastric nerve is composed of posterior filaments united to nerves proceeding from sympathetic ganglions.* It has been shewn that secretion is equally injured by the division of either, consequently in this respect their functions are the same. I am therefore justified in concluding, not only that every posterior nerve is concerned in performing secretion, but also that the economy of the sympathetic ganglions has more relation to the means whereby the motion of the parts supplied by them may be constant and independent of the will, than to any difference of discernment function.

* See the classification in Chap. II.

SECTION IV.

*On the CAUSE of the EVOLUTION of ANIMAL
HEAT.*

THE only question now remaining to be disposed of, before shewing the precise manner in which I conceive the nervous system is employed in maintaining all the vital functions, is in what light animal heat should be regarded. Is it a secretion? In the preceding section I have said, that every thing which results from the action of nervous influence upon the blood ought to be considered as such. Let us then inquire whether animal heat comes under this definition.

The constant loss of caloric by radiation and communication to surrounding objects of a temperature lower than that of living animals, leaves no room to doubt but that there is some power in living bodies by which it can be, and is being, constantly reproduced. It is not so much my object to point out the insufficiency of the old chemical theories, all of them are wide of the truth, though some attest the ingenuity of their authors, as it is to adduce positive evidence of the agency of the nervous system in effecting this process, and I trust it will

be found that the labours of some late physiologists have left nothing farther to be wished for on this subject.

The experiments of Mr. Brodie, published in the Philosophical Transactions for the years 1811 and 1812, and which either are or ought to be too well known to require to be individually pointed out, are regarded as affording direct evidence that the process, by which the temperature of the body is maintained, is not performed in the lungs, and at the same time as proving, that the nervous system is in some way or other essential to the evolution of caloric. Dr. Philip has advanced a step farther in this inquiry, and proved, that by lessening the extent of this system, by destroying part of the spinal marrow, the evolution of caloric is materially diminished, and has considered this process as constituting a part of the secretory function, because, like all secretions, it appeared to be the result of the action of the nervous influence upon the blood. He was thus led to make the following statement—"Among the secretions I ranked the evolution of caloric, although not taking place upon any particular surface, because it appeared to be performed by the same power acting upon the same fluid, (*i. e.* the blood,) and because, like secreted fluids, it fails when any considerable part of the

influence of the brain or spinal marrow is withdrawn.*

The experiments of M. Chossat of Geneva bear still more directly upon this interesting subject, and besides confirming the opinions of Mr. Brodie and of Dr. Philip, they afford such convincing evidence of the agency of the nervous system in causing the evolution of caloric from the blood, as renders the fact no longer doubtful. I have been obliged to avail myself of these experiments as they are quoted by M. Adelon in his work entitled *Physiologie de l'Homme*,† on account of the original memoir being out of print. M. Adelon, after alluding to the experiments of Mr. Brodie, Dr. Philip, and others, says, “M. Chossat, d'après des expériences de même genre, veut préciser davantage quelle partie nerveuse préside au dégagement du calorique. 1°. A l'aide d'une ouverture fait au crâne d'un animal, ce physiologiste coupe le cerveau au-devant du pont de Varole, de manière à ce que la huitième paire de nerfs étant intacte, et la respiration se continuant naturellement, on n'ait pas besoin de recourir à l'insufflation pulmonaire. Dans cet animal ainsi mutilé, la respiration et la cir-

* *Experimental Inquiry*, p. 156.

† *Tome 3eme*, p. 416.

culatation continuent ; l'expérimentateur s'assure que c'est du sang artériel qui circule dans les artères ; et cependant la température de l'animal baisse graduellement ; de 40 degrés (102 Fahr.*), quelle était au commencement de l'expérience, elle tombe à 24 degrés (57 Fahr.) en douze heures, époque à laquelle l'animal meurt. Il paraît donc évident à M. Chossat qu'à compter du moment de la section du cerveau, il ne s'est plus dégagé de chaleur, et que le corps s'est refroidi graduellement comme il eut fait après la mort. Bien plus, le temps où le refroidissement s'est fait le plus rapidement est celui où la circulation était la plus active, c'est-à-dire le commencement de l'expérience. Si au lieu d'expérimenter de cette manière, M. Chossat paralysait l'action cerebrale par une forte commotion, ou en injectant une forte décoction de l'opium par la veine jugulaire, et s'il remplaçait alors la respiration par l'insufflation pulmonaire, les resultats étaient absolument les mêmes. Il conclut donc que *le cerveau influe prochainement sur la production de la chaleur*. Il s'agissait dès lors de savoir *par quel intermédiaire*, si c'était par la huitième paire, ou par la

* No mention is made of what thermometer was used, but I suppose it to have been the Florentine, because in that only does the 40 correspond to 102 of Fahrenheit, which is the usual temperature of animals.

moëlle spinale. 2°. Il coupe les nerfs vagues à un chien, et adaptant une tube à la trachée-artère pour que la respiration se continue, il voit néanmoins la température de l'animal baisser graduellement, et n'être plus après 60 heures, époque à laquelle l'animal meurt, qu'à 20 degrés (48 Fahr.) L'animal n'était pas mort d'asphyxie, car ses poumons étaient crepitants, sans aucune trace d'infiltration, et en partie pleins de sang artériel. Selon M. Chossat il est mort de froid, ce qui fait une nouvelle cause de mort à ajouter à toutes celles qui ont fait la section ou la ligature de la huitième paire de nerfs. Cependant, comme dans cette expérience, l'abaissement moyen a été moindre que dans les précédentes, M. Chossat pense qu'il s'est dégagé encore *un peu de chaleur après la section de la 8ème paire, tandis qu'il ne s'en était plus dégagé du tout après la section du cerveau.* 3°. Il coupe la moëlle spinale sous l'occiput, et bien qu'il pratique l'insufflation pulmonaire, il voit aussi la température de l'animal tomber graduellement et la mort arriver après dix heures, lorsque la température est baissée à 26 degrés (65 Fahr.) Comme la mort arrive dans cette expérience bien plus promptement que dans la précédente, il conclut que *c'est plus par la moëlle spinale que par la huitième paire, que s'exerce l'influence du cerveau sur la production de*

la chaleur. 4°. Enfin, comme lorsque la moëlle est coupée entre chacune des douze vertèbres dorsales, l'abaissement se montre d'autant *moins rapide, que l'opération est faite plus bas* ; comme même il paraît nul pour les dernières ; M. Chossat pense que ce n'est point par elle même, mais par le trisplanchnique qu'agit, la moëlle. Pour s'en assurer, il ouvre l'abdomen à un animal à gauche, au dessous de la douzième côte, et extirpe la capsule surrénale de ce côté. Dans cette opération, le trisplanchnique est coupé au lieu où ce nerf se jette dans le plexus semi-lunaire. Or, dans cette expérience, l'animal perd graduellement sa chaleur, et meurt après dix heures, dans le même degré de refroidissement que lorsque on avait coupé la moëlle spinale sous l'occiput. Cependant, comme il n'y avait eu de coupé qu'un des nerfs trisplanchniques, pour équivaloir à la section des deux, M. Chossat fait cette autre expérience ; par une incision faite dans le dernier espace intercostal, tout près du rachis, il va lier l'aorte au dessous du lieu où elle traverse l'arcade du diaphragme, et une tube est inséré dans la trachée-artère pour prévenir l'asphyxie : l'animal perd encore plus rapidement sa chaleur, et meurt après cinq heures. Dans tous ces cas l'animal est, selon M. Chossat, mort de froid ; l'action à laquelle il doit de renouveler

le calorique que lui soutire continuellement l'élément ambiant, a été rendue impossible. Pour avoir une terme de comparaison, il avait fait mourir de froid des animaux par une immersion prolongée dans l'eau froide, et il avait vû de même leur température baissée jusqu'au degré qui est incompatible avec la vie, et qu'il dit être le 26 (65 Fahr.) pour les animaux à sang chaud. Appelant l'attention sur les cas naturels de mort par congelation, il dit qu'évidemment le froid tue par l'épuisement des forces nerveuses, comme l'indique l'acroissement progressif de la stupeur et de la debilité des principales fonctions de l'économie. Enfin, tuant tout-à-coup un animal, il a vu qu'elle était tout-a-fait la même que dans les cas ou il avait lésé directement le cerveau, ou coupé sous l'occiput la moëlle spinale ; ce qui l'a confirmé dans la pensée que dès lors il ne s'était plus dégagé de chaleur."

Exp. B. In addition to this evidence I have that of an experiment which I made before I was aware of the publication of those of M. Chossat. It was suggested by observing that in the experiments of Dr. Philip, after the destruction of the spinal marrow, the temperature was ascertained by introducing the bulb of the thermometer into the rabbit's *mouth*, and that these experiments did not show an uniform

diminution of heat.* It then occurred to me that if the evolution of animal heat depended upon any influence derived from the brain, the parts *below the division* of the spinal marrow ought to be those which would suffer the greatest diminution of caloric upon the destruction of part of that organ, and consequently, that the bulb of the thermometer ought to have been introduced into the rectum rather than into the mouth, as being more likely to afford an accurate result. The experiment was therefore performed in the following manner:—In a rabbit between six and seven weeks old the posterior part of the rings of the second and third lumbar vertebræ were removed, the spinal marrow was then divided, and the part below the section destroyed very slowly, so as to excite a great many contractions in the voluntary muscles which received their nerves from it, in order that the nervous influence might be exhausted as much as possible. Before the commencement of the experiment, the thermometer introduced an inch into the rectum, indicated a temperature rather above 103° , in $50\frac{1}{2}$ hours it had fallen to 85° , gradually decreasing, as appears to have been the case in the experiments of M. Chossat. The rabbit was allowed to eat and drink, and care was

* Experimental Inquiry, pp. 142, 143.

taken to evacuate the bladder by gentle pressure on the abdomen from time to time. At the end of the $50\frac{1}{2}$ hours he was so weak, that it was thought proper to put an end to his misery by a blow on the occiput. The following are the particulars as noted at the time:—

At five minutes before 12, tempera-	
ture of rectum	103°.
At ten minutes after 3 p. m. . . .	96 $\frac{1}{2}$ °.
At thirty minutes before 6 p. m. . .	96 $\frac{1}{2}$ °.
At ten minutes after 12 at night . .	96 $\frac{1}{2}$ °.
At ten minutes before 11 a. m. . . .	96°.
At thirty minutes after 5 p.m. not quite	95°.

It was now wished to compare this with the temperature of the mouth, but as there was some difficulty in keeping the thermometer in the mouth long enough to judge, rather than worry the animal, the bulb was placed under the jaw and the head pressed towards the neck: the temperature then indicated was 98°. At thirty minutes after 2 p. m. of the next day the temperature of the rectum, taken as before, was 85°: between the shoulder and ribs, 89°.

Although in an experiment so severe as the foregoing, the injury caused by the slow destruction of so large a portion of the nervous system is so severe as to affect very considerably the functions of the whole of that system;

still the difference between the temperature of parts whose nerves belonged to the portion of the spinal marrow above the division, and of those below was sufficiently remarkable. This being the case, I did not think it right to continue experiments attended with so much suffering to the animals, which however frequently repeated or varied could do no more than establish the general fact, that the evolution of caloric in living bodies is the result of an action of the nervous influence upon the blood; and consequently confirm the opinion of Dr. Philip, that this process ought to be ranked as part of the general secernent function, however such an idea may be at variance with preconceived and vague opinions founded on the old chemical theories, which are acknowledged on all hands to be insufficient.

Without thinking it necessary to agree with M. Chossat as to his having discovered cold to be a new cause of death, if we keep to the simple fact, that although arterial blood may circulate through the body, yet if when the communication of any part with the brain is cut off the evolution of caloric ceases, and it is shewn by other experiments that the formation of every secretion is always impaired by the same injury, it seems impossible to arrive at any other conclusion than that caloric is as

much to be considered a secretion, as bile, saliva, or gastric juice.

It is farther to be observed, that as the diminution of heat and the approach of death always bore a relation to each other, it is fair to infer that the cause which is active in effecting the disengagement of caloric, is the same which maintains the vitality of the body generally; and also that this cause, as is shown more particularly in the section of the brain and different parts of the spinal marrow, is something which proceeds from the former along the latter to the extremities of its nerves. These inferences become extremely interesting when viewed in connexion with the opinion advanced in the preceding section, viz. that the second and fourth as well as the third orders of nerves are concerned in the secernent function, for in that case, it is evident, that wherever the extremities of nerves and capillary vessels are found, there will caloric be evolved from the blood.

The general fact of the universal dependence of the secernent function upon the nervous system being established, no farther information can be obtained by a repetition of experiments of the same nature, and the only clue to the discovery of the *modus operandi* of

the nervous influence is to be found in making a proper use of our knowledge of the distinct properties of the several nerves.

SECTION V.

On the MODE in which by means of the Nerves the CEREBRAL INFLUENCE is rendered available in maintaining the MOTOR and SECERNENT FUNCTIONS of the Animal Economy.

IT is now generally admitted, that those nervous filaments which belong to the anterior columns of the spinal marrow are destined to convey the influence of the will to the muscles of voluntary motion; and that those belonging to the posterior columns are destined to convey impressions from their extremities to the brain. For the knowledge of these individual properties of the second and third classes of nerves we are indebted to the labour and researches of Sir C. Bell and M. Majendie.

Their experiments are regarded as having established the following facts. First, that when the anterior filaments are divided, the posterior remaining uninjured, all voluntary

motion ceases in the parts which were supplied by them. Secondly, that when the posterior filaments are divided, the anterior remaining uninjured, sensation is lost although voluntary motion is unimpaired. The same facts are also proved by experiments upon the anterior and posterior columns of the spinal marrow. It is thus plainly demonstrated by these experiments, that the influence of the will or of the brain cannot be transmitted to the voluntary muscles through the posterior columns or the nervous filaments belonging to them; and that external impressions cannot reach the brain through the anterior columns or the filaments arising from them: any influence or impression so conveyed by either set of nerves would be retrograde, and contrary to their usual and fixed mode of action, from which they cannot deviate. There is no action therefore in an anterior filament except *from the brain to its extremity*; neither is there any action in a posterior filament except *from its extremity to the brain*.

As these nerves thus bear an opposite relation to each other, I think the expressions, *arising from the anterior and terminating in the posterior columns of the spinal marrow*, are well adapted to denote the particular mode of action which each set of filaments possesses, precisely

as arteries and veins are said to arise from and terminate in the heart, which terms are considered as indicative of the direction of the current of the blood through them. Upon this view of the action of the nerves, each set of filaments is to be considered as perfectly and entirely distinct in their functions throughout their whole course, as if each were actually separate, however intimately they may be mixed together. This point, however, has been sufficiently insisted upon by Sir C. Bell in his work on the physiology of the respiratory nerves.

These facts are of great importance in explaining the mode in which the cerebral influence arrives at secreting organs. The experiments of Dr. Philip, 58, 59 and 60, which have been already slightly alluded to, prove that the secreting power of the stomach is deranged by *destroying* a part of the spinal marrow as well as by cutting out a portion of the pneumogastric nerves. (The reason why the simple division of the spinal marrow, as in Exp. 62, does not cause so much injury to the digestive process as is caused by its destruction, will appear presently.) A question of great interest arises out of these experiments: it is this; since the influence of the brain is prevented from maintaining the secernent function of the stomach in either case, by which of these two channels

is it that the influence of the brain arrives at the stomach? Is it through the spinal marrow, or through the pneumogastric nerves? or is it conveyed through both of these communications? It is in determining this question that the knowledge of the individual properties of the second and third classes of nerves, as demonstrated by Sir C. Bell and M. Majendie, becomes so valuable, because the simple fact of the loss of secreting power being the result of either injury throws no more light upon the mode in which this function was performed by the nerves, than the division of either the positive or negative wire of a galvanic battery, while in action, does upon the direction in which the current of electricity was passing through them. All that can be known from either fact is, that an action which was going on, and in which both played an important part, has been stopped. The opinion which appears to be most prevalent among physiologists at the present time is that the influence of the brain is conveyed to the stomach along the pneumogastric nerves. This opinion however, so far as I am able to judge, has arisen more from these nerves having been so much used in experiments instituted to determine how far secretion is dependent on the nervous system, than from any proper consideration of the individual

properties of the nerves themselves ; because the same opinion would have been held with regard to the spinal marrow, if this part of the nervous system had been experimented upon frequently instead of the pneumogastric nerves. The most probable reason why the spinal marrow has not been more frequently experimented upon to ascertain what influence it has upon the stomach, is, that from its being inclosed in a bony canal, it is difficult of access. Dr. Philip thinks that the stomach is supplied through both channels ; in fact, the 58th, 59th, 60th, and 62d, are adduced expressly to prove it. The reader will recollect that M. Chossat wished to determine, whether the power by whose agency the evolution of caloric (which is nothing more than a secretion) was effected, proceeded from the brain along the spinal marrow, or the pneumogastric nerves, and came to the conclusion that it appeared to be through the former rather than the latter. This opinion of M. Chossat's is greatly confirmed by the fact that the pneumogastric nerves, at their exit from the skull, are composed entirely of posterior filaments, and have protuberances upon them like every other nerve which terminates in the posterior columns of the spinal marrow ; they therefore belong entirely to the third class, along which no influence or impres-

sion can be conveyed except from their extremities towards the brain, and consequently this is exactly the opposite direction in which the influence which the stomach receives from the brain, is at present supposed to flow. This being the case, it is impossible for any influence to proceed from the brain except along the anterior columns, and the second class of nerves which pertains to them. Here then arises a difficulty which has never been considered at all, simply because no one has hitherto been aware of its existence. It has been clearly proved that the discernment function of the viscera, as well as the motion of the heart and intestines, is entirely dependent upon an influence derived from the brain, yet none of these organs are provided with any nerve through which it can directly arrive at them. This difficulty does not exist with regard to the means by which the motion of voluntary muscles, and the nutrition of the limbs and general frame of the body may be supported, because all these parts are plentifully supplied by the second class of nerves, so that the influence of the cerebrum, to which this class pertains, may freely pass through them to all parts to which they are distributed. That the heart and the muscular fibres of the stomach and intestines have no anterior nerve is fully proved

by their action being independent of the will. If these parts were supplied in the same way as the voluntary muscles, they would also, like them, become subject to fatigue and require rest; in which case it would be quite impossible that they could continue in action constantly from birth till death. Here again is another difficulty, which is entirely overlooked by Dr. Philip and those who support the opinion that the action of the heart is constant, purely because its stimulant is constantly renewed by the uninterrupted flow of blood; the black blood being the proper stimulant to the right, and the red being the proper stimulant to the left side of this organ. This branch of the doctrine of irritability, on account of its plausibility, has obtained very general assent, and is urged with great appearance of probability as far as regards the heart, stomach and intestines; but it does not even explain the action of the heart when removed from the body, and much less the constant and involuntary action of those muscles which have no renewed application of a stimulant, as, for example, the orbicular muscle of the mouth, the levatores, and sphincter ani. The action of these muscles may be called constant with more propriety than even that of the heart, because it is without intermission; and although

the will has the power of increasing their action, it has none whatever either of relaxing or preventing it. A person is able to increase the action of the orbicular muscle of the mouth, as in the act of whistling, or to overcome its action for a time by means of those which are attached to each corner of the mouth; but as soon as the will ceases to act upon these muscles, the orbicular muscle resumes its former state. So also if a person attends to what occurs when the contents of his bowels are evacuated, he will find that the action of the levatores and sphincter ani muscles is not relaxed, but is overcome by the united power of the diaphragm and abdominal muscles. It is however satisfactory in this, as in former instances, to be able to refer to Dr. Philip's own experiments for a refutation of a part of this opinion.

The reader will recollect that in Exp. 5, in which, after a great part of the brain and the whole of the spinal marrow of a rabbit were destroyed by means of a small hot wire, artificial respiration not being performed, "black blood was thrown out copiously *per saltum*" upon opening the carotid artery: thus demonstrating most clearly that the left side of the heart will contract quite as well upon black as upon red blood. This fact is shewn even still more satisfactorily in Exp. 7, in which "the spinal

marrow was wholly destroyed by a hot wire, and artificial breathing was not performed previous to opening the artery, from which dark-coloured blood flowed *per saltum*. The lungs were then inflated, and florid blood soon began to flow copiously from the vessel, and appeared like a red stream mixing with the dark-coloured blood which had previously come from it. This experiment was repeated in the same manner and with the same result."

It is not a little extraordinary that physiologists should apply a stimulant to a voluntary muscle, and find its power exhausted in proportion to the number of contractions thus excited, and yet consider no contradiction implied in saying, that the constantly renewed application of a stimulant to the heart does not in like manner equally exhaust its power. If, however, this doctrine be true, any voluntary muscle might act constantly provided its stimulant was so applied, whereas we know full well that the muscles of the arm or leg of the most powerful man cannot be made to contract seventy times in a minute even for four-and-twenty hours together, though the heart of the most feeble individual continues its motion without fatigue for a series of years. As it has been shewn that the contraction of the muscular fibre is not independent of the nervous

system, it will readily be perceived that this doctrine, in accounting for the immediate cause of the heart's action, passes over the more difficult and important question, namely, how this action may be supported; and has prevented physiologists from drawing the more correct inference from the evidence before them, which is, that *as the heart, in consequence of its repeatedly renewed excitement, performs more contractions than a simply voluntary muscle, it must necessarily have some means of being more plentifully supplied with a power by which it may be enabled to contract.* The true reason why there should be a peculiarity in the nerves supplying muscles of involuntary motion now becomes obvious, and it only remains to be inquired in what manner the cerebral influence may be conveyed to the sympathetic, after it has passed into the anterior nerves. This is the great difficulty which nothing but the knowledge of the individual properties of the nerves can solve. There is no action, as has been already shewn, in anterior nerves, except *from* the cerebrum, nor in the posterior, except *towards* the cerebellum; and though in juxta-position they are perfectly distinct, what then is there to prevent the passage of the cerebral influence from the extremity of an anterior to that of a posterior nerve, or to hinder it from passing

along the latter in a reflux direction? If this be fact, the reason why arteries receive an increased supply of nerves in proportion as their size diminishes is at once explained, and the whole secernent function of the limbs and general frame of the body, wherever the second order of nerves is distributed, is completely provided for; and our knowledge of the fact, that galvanism exerts its influence upon chemical substances in passing from the positive to the negative pole, enables us fully to comprehend the precise mode in which the cerebral influence is rendered available for the accomplishment of the whole of that part of the secernent function upon which the growth and nutrition of the body depends.

The cerebral influence having passed into the posterior nerves, will return along them to their terminations, and thus complete a circuit which bears a close analogy to that which is performed by the blood in the arteries and veins, and which may justly be termed the CIRCULATION of the NERVOUS SYSTEM. As I know of no reason which should induce me to suppose that this circulation does not exist, nor of any physiological fact or experiment which proves that it is incompatible with the more obvious functions of the nerves relating to volition and sensation, and therefore

cannot take place, I think I am justified in believing that there is a circulation in the nerves of a subtile fluid somewhat similar to electricity, passing as regularly and uninterruptedly along them from and to the brain, as there is of blood in its vessels from and to the heart.

As the returning current flows along the posterior or third class of nerves, part of it will pass by means of the ganglions which belong to each of these nerves to the supply of the sympathetic ganglions: the rest, in continuing upwards along the posterior columns of the spinal marrow, will supply the two spinal accessory nerves in its passage to the cerebellum. This reflux current ought, as has been already stated at page 54, to be distinguished from that which flows from the cerebrum by the term *Nervous Influence*, not however as signifying that any change is supposed to take place in the *Cerebral Influence* in any respect analogous to the change which the blood undergoes in its passage from an artery into a vein, but merely to express the reciprocal correspondence which exists between the brain and nerves; and also to signify that the sympathetic ganglions are supplied from the nerves, and not by any direct communication with the original source from whence this influence is derived.

It has been conjectured by some physiologists that the ganglions on the posterior nerves prevent volition from having any power over the sympathetic nerves, while they allow nervous influence to pass to the parts supplied by them. Before, however, this conjecture can be admitted, it must be proved that the experiments of Sir C. Bell and M. Majendie, to which I have already alluded, are either incorrect or admit some other interpretation. It is farther to be observed, that as these bodies offer no impediment whatever to the transmission of sensations, there is no reason to believe, provided volition had power over any nerve pertaining to the posterior columns of the spinal marrow, that these bodies could prevent the will from affecting the action of the heart. Since these posterior ganglions do not interfere with the known functions of the nerves to which they belong, it is fair to conclude that they perform some office which has not hitherto been understood, and which must be that of enabling every posterior nerve to contribute its portion of influence for the support of the motor and secernent functions of all the viscera. The office of the sympathetic ganglions thus appears to be nothing beyond that of collecting and transmitting the influence they receive from the posterior ganglions. The only opinion which presents itself as interfering with what

has just been stated is, that the sympathetic ganglions may be able to form their own nervous influence; but as this idea rests upon no evidence, it is unnecessary to say more than that if this were so, no reason can be given why the sympathetic should be connected with the spinal nerves at all, because the effects of strong mental emotions are, as has been already shewn, rather deleterious than otherwise, both in rendering the action of the heart irregular, and in injuring the whole digestive process.

When the nervous influence has passed into the sympathetic ganglions, it will flow along every nerve proceeding from them, and supply the heart, stomach, and intestines with the means of maintaining their constant motion; while in passing from the extremities of these nerves to those of the pneumogastric, in the same way as it had already passed from the anterior to the posterior, the whole secernent function of the viscera is in like manner provided for. The muscles of involuntary motion are thus supplied with nervous influence to an extent far exceeding that which voluntary muscles receive, and are consequently enabled to continue their motion without fatigue for an indefinite length of time; and they are entirely independent of the will, because *its power cannot possibly extend beyond the extremities of the anterior nerves.*

The reader is now also fully prepared to understand that the reason why the heart's motion is continued for a short time, or may be maintained for a longer period, by the performance of artificial respiration after the brain and spinal marrow have been removed, is because the nervous influence conveyed by the posterior nerves passes to the sympathetic ganglions *before these nerves terminate in their respective columns of the spinal marrow*, and consequently the supply of nervous influence is not in the least immediately interfered with. We thus perceive that another of the inferences which Dr. Philip has drawn from some of his experiments, namely, "that we have reason to believe that the great sympathetic nerve arises from the spinal marrow," is erroneous.

The circumstances which led me to entertain this opinion respecting the mode in which the sympathetic ganglions receive their supply of nervous influence, are chiefly as follows:—Not being satisfied with the inferences of Dr. Philip, relating to the independent nature of the "muscular power," I wished to ascertain what would be the difference, if any, in the length of time during which the heart would beat in an animal whose head had been cut off and spinal marrow slowly destroyed, so as to affect the spinal nerves as little as possible and with-

out communicating any shock to them as by crushing it, when compared with one simply decapitated, the spinal marrow remaining untouched and artificial respiration being kept up in both.

Exp. C. The animals (young rabbits) being carefully selected as to age and size, were prepared in the following manner:—A strong needle, carrying a ligature of silk, waxed and several times doubled, was passed behind the trachea, which was brought as forward as possible that all the blood vessels might be behind the ligature, which was then tied very tight at the back of the neck in order to prevent the effusion of blood, and the head immediately cut off; the tube of a pair of bellows, suited to the purpose, was then fixed to the trachea, which, not being included in the ligature, was perfectly free, then the spinal marrow was either left uninjured or slowly destroyed by passing down its canal, first a wire, and afterwards a thin slip of whalebone, as far as the tail.

The results obtained from these experiments varied in almost every one; the cause of which could not be imagined, though the hearts of those whose spinal marrow had been destroyed never continued to beat quite so long as those in which this organ was left untouched, the difference being in some cases only four or

five minutes, which was not considered sufficient to warrant any decisive conclusion. They were therefore repeated with great precaution to avoid, if possible, every cause of error; still the same differences attended the results, and the cause of them remained in as much obscurity as ever.

After about a week's reflection bestowed upon the endeavour to unravel this perplexing mystery, it was recollected, that in some of the experiments a few minutes elapsed before the spinal marrow was destroyed, and in those rabbits comparatively *few contractions* were excited in the voluntary muscles during the destruction of the spinal marrow; and it was also recollected, that it was in these that the action of the heart continued nearly as long as in those where it was not destroyed at all. This was eagerly seized upon as the only circumstance that appeared at all likely to afford a solution of the difficulty; because as every contraction of a muscle exhausts a certain quantity of nervous influence, it was concluded that the universal contractions of the voluntary muscles, excited by the destruction of the spinal marrow, *immediately after decapitation*, had consumed, exhausted, or been supported by that which would otherwise have supported the contractions of the heart. In order to ascertain this

point, these experiments were again repeated with the assistance of Mr. Richard Jackson, a gentleman well known among his fellow students at St. Bartholomew's Hospital for his perfect knowledge of anatomy.

Exp. D. The rabbits were prepared as before. Immediately after decapitation, the tube was fixed to the trachea, the lungs inflated, and no time lost in destroying the spinal marrow, which excited violent contractions in every voluntary muscle. The heart ceased to contract with any thing approaching to vigour and regularity in less than five minutes, but continued acting feebly, and gradually less frequently, until about twenty-six minutes from the time of decapitation.

Exp. E. A second rabbit was now similarly treated, but in passing the wire down the spinal canal its point hitched against one of the dorsal vertebræ, by which about half a minute was lost, the contractions of the muscles were not quite so violent as in the first. The action of the heart ceased in about thirty-three minutes from the time of decapitation.

Exp. F. A third rabbit was then treated as the two first, with this exception, that the spinal marrow was not destroyed until *ten minutes* had elapsed, when only a trifling rigidity of the voluntary muscles was produced,

and the heart continued beating a considerable time as when the spinal marrow was not destroyed, at length becoming more feeble, and ceasing at fifty-four minutes from the time of decapitation.

It was remarkable that in the third rabbit the contractions of the heart were much more perfect than in the others, up to the end of the experiment: the whole of the fibres of the ventricles seemed to move at once, accompanied with an elevation of its apex, which was not observed in either of the other two to continue above five minutes after the destruction of the spinal marrow; after this time, in the first and second rabbits, the contractions which occurred appeared to begin in some fibres of the ventricles before the others, so that they approached somewhat to what might be termed vermicular contractions. It thus appears that the action of the heart is weakened in proportion to the number of contractions which are excited in the voluntary muscles. Now as we know from other experiments that every muscular contraction exhausts a certain quantity of their power, the only probable explanation of the above mentioned fact is, that the contractions excited in the voluntary muscles by the destruction of the spinal marrow had exhausted the nervous influence, which would otherwise have supported the action of the heart. These experi-

ments are not brought forward as proving any thing more than what might have been already known, had physiologists paid proper attention to a circumstance with which all must be familiar, namely, the languid action of the heart and arteries which is consequent upon long continued and unusual exertion. It is true that moderate exercise, by accelerating the motion of the blood in all parts of the body, increases the action of the heart more or less according to the healthy constitution of the individual, which continues for some time after the body has been at rest; but if the exercise be pushed to the extreme of weariness and the muscles begin to be incapable of obeying the commands of the will, the vigour with which the circulation was carried on is decidedly impaired. When the exertion is pushed to a still greater extent the heart's action ceases altogether. This is exemplified in the cases of those soldiers who die from fatigue upon a long march, where the time allowed for rest is not sufficient. Horses also but too frequently die from this cause after a hard day's hunting.

The secernent as well as the motor function of these nerves suffers equally from the excessive action of the voluntary muscles, of which an example is furnished by an experiment instituted by Mr. Hunter, in which two pointers

were fed with an equal quantity of mutton, after which one was taken out to hunt, the other was allowed to rest quietly asleep. In the one which had been employed in hunting, the food had undergone but little change; in the other it was found to have been completely digested.

It then occurred to me that if this opinion respecting the mode in which the sympathetic ganglions are supplied was correct, the action of the heart ought to be increased by the application of a stimulant to the surface of the body, where the extremities of so many nerves are distributed, as well as when applied to the brain, as in Dr. Philip's experiments.

Exp. G. The heart of a frog was exposed to view, performing fifteen beats per minute. The legs and thighs were then immersed in spirit of wine. The heart immediately began to beat more quickly, and in a little more than two minutes was beating twenty-two times per minute. This was repeated several times with a similar result. The extent of surface stimulated was necessarily limited to the lower extremities, lest by accident any of the spirit should get into the wound made to expose the heart and thus vitiate the result, the increased action however from the stimulation of so small a portion of the nervous system was still suffi-

ciently remarkable both in quickness and in strength.

This experiment again is nothing more than an illustration of facts already known, as for instance, when a considerable extent of the surface of the body is scalded by hot water, but not so much as to cause the skin to slough, the nerves are greatly stimulated, as is shewn by the pain experienced, and an increase of the action of the heart is the result. When on the contrary the skin is extensively burned by fire the nerves are destroyed, consequently the heart is deprived of the nervous influence which it was receiving from them, therefore it is, that persons so injured make no particular complaint but very generally fall asleep, the heart beats feebly and at length ceases gradually. The effect of the intense cold to which travellers are sometimes exposed in snowy regions is a farther confirmation of this opinion. The cold is applied to the nerves on the surface of the body and paralyses their action, the posterior lose that of communicating external impressions to the brain and the will cannot exert its power over the muscles; such persons are commonly said to be benumbed, they fall asleep and die. It thus appears from these facts that whether the nerves are actually or virtually destroyed, or whether the nervous influence is

exhausted by the action of the voluntary muscles, the effect upon the heart is precisely the same. If on the contrary the flow of nervous influence is increased by a stimulant the action of this organ is greatly invigorated.

While the object of the wandering course of the pneumogastric nerves is thus satisfactorily explained, that of the recurrent nerves is also rendered evident. It is not on account of any sympathy, but because the parts to which they are distributed may be able to maintain their long continued motion by means of the branches from the inferior cervical sympathetic ganglion, with which they are accompanied.

The nervous influence, after having provided for the secernent function of the viscera, in passing to the extremities of the pneumogastric nerves, will flow along them to their terminations, and send a supply to the superior cervical ganglions of the sympathetic by means of the communications which exist between them and those ganglions.

These views concerning the functions of the brain, nerves, and posterior ganglions, render every fact observed by Dr. Philip with regard to the effects of substances applied to the brain and spinal marrow perfectly intelligible. In the first place, stimulants and sedatives, when applied to either of these parts, increase or

diminish the action of the heart, because they accelerate and retard the supply of nervous influence. Even the difference observed between the immediate operation of the one, and the less rapid effect of the other is thus explained. By increasing the supply the action is increased in the same ratio, whereas when it is diminished, the action of the heart will not become more feeble until its contractions have exhausted the influence which was in its nerves at the time of the application of the sedative.

Secondly. The functions of the viscera are least interrupted by the simple division of the spinal marrow, because there are but few contractions of the voluntary muscles excited by such an injury; and as they can then no longer be excited by volition, none of the influence in the nerves connected with the divided part can be exhausted by their action; consequently a great proportion of this influence will continue to flow to the support of the motor and secernent functions of all parts supplied by the third order of nerves.

Thirdly. The slow destruction of the spinal marrow, effected by moving a small wire up and down its bony canal, affects the functions of the parts supplied by these nerves to a great extent, because many and violent contractions of the voluntary muscles are thus excited. The

reason why the functions of these parts are not immediately put a stop to by this mode of destruction, is to be found in the fact that the voluntary muscles may still be excited to contract, which shews that the whole of the nervous influence cannot be exhausted in this way; this again is explained by the fact that when one part of a nerve is destroyed by pinching it to excite muscular contractions, a part nearer to the muscle must be stimulated in order to cause any farther contractions in it. Since therefore the whole of the nervous influence cannot be exhausted by the slow destruction of the spinal marrow, some small quantity will still continue to be sent by the posterior nerves to the sympathetic ganglions, because they impart their influence to these bodies before they reach the spinal marrow which has been destroyed.

Fourthly. The greatest and most universal injury is that which is caused by crushing the brain, because a violent shock is communicated to every nerve in the body, by which they are totally paralysed. The continuance of the motion of the heart of a frog after such an injury must depend upon some difference in the functions of the nervous system in cold blooded animals, with which we are as yet unacquainted, though I am far from thinking that even this

will long remain a mystery. There is however another circumstance, which, although hitherto inexplicable, is now unfolded to our view, namely, the reason why the viscera are generally the first to become putrid after death. When a person is killed by any thing which acts suddenly upon the nervous system, and by annihilating the whole of its functions at once, the muscles do not become rigid, nor does the blood become coagulated, and the whole body is observed to pass very rapidly into a state of decomposition. The same fact obtains in those persons or animals whose death is caused by the action of several poisons, which produce their baneful effects upon this system to as great an extent, though not quite so instantaneously. But when death results from any common cause the voluntary muscles remain at rest, and there is nothing to affect the influence of their nerves; the intestines on the contrary continue their peristaltic motion for a considerable time, in fact until nearly the whole of their nervous influence is exhausted. When this motion has ceased, the state of the intestinal canal very nearly approaches the condition of parts whose nervous influence has been destroyed or exhausted by prussic acid or by a flash of lightning, consequently the time at which the process of decomposition will com-

mence will be pretty nearly the same, varying a little of course according to what has been the cause of death, but the muscles of the trunk will not be affected by this process until several days later.

CHAPTER V.

*On the FUNCTIONS of the FOURTH ORDER
of NERVES.*

THE reader's attention has as yet been chiefly directed to the consideration of voluntary and involuntary muscles, and he has seen that one of the great distinctions between these two classes of muscles is, that the one set becomes fatigued, and requires rest after the performance of a few contractions, while the other continues in constantly repeated action, with no interval of repose for a series of years. Besides these, there is a third class of muscles, the action of which is continual, and is entirely subject to the regulation of volition, although they never require rest. The power of the will over these muscles is only exerted occasionally as convenience or necessity may require, and is employed solely in increasing their action; but volition has no more power of stopping or preventing their action, than it has of interfering with that of the heart. The intercostal muscles belong to this class, but the will has no power

of even interrupting their action, except by increasing it to the highest degree of which they are capable, as in fixing the ribs, or by calling into action another set of muscles, by which they may be counteracted, as by closing the glottis. It thus appears that the will has in no case any power of preventing or interrupting the action of any muscle whatever; in the purely voluntary, it excites them and determines the precise degree, in the mixed, it increases occasionally, according to circumstances, the action which is already going on.

The muscles of the face as well as the intercostals belong to this set, which ought to be considered as constituting a third class; their constant action however, is not so apparent in health, as after an apoplectic seizure, when one half of them are paralysed. In such cases the constant action of these muscles is particularly well observed, because the angle of the mouth is drawn towards the sound side by the unaffected muscles, and fixed there permanently. Every one must have observed, that the will has no power whatever of relaxing the action of the muscles attached to the angle of the mouth even for a single moment, so as to allow it to return to its natural position, though volition has at all times the power of increasing their action, and distorting the mouth to a greater ex-

tent by drawing its angle still nearer to their points of attachment. It must be borne in mind, that the distortion of the mouth is not the result of any new action on the part of these muscles as a consequence of the paralysis, but simply a continuance of what they had always been engaged in, and only rendered more plainly perceptible by the loss of the power of their antagonists. Nothing parallel or analogous to this is observed when the muscles of the arm are partially paralysed ; on the contrary, when the power of the will is not exerted over those which it is still capable of exciting, the paralysed and the sound muscles are all alike in a state of rest.

If the muscles of the eyes, face, and ribs had not this power of constant action, and were purely voluntary, the lids would drop, and always remain so, as in the affection termed ptosis, except when raised by volition ; the mouth would also be always relaxed, and the saliva be constantly dribbling from it as is seen in very old persons, in whom every vital function is feebly performed, except when its muscles are called into action by the will, consequently no regularity of expression could be maintained. If the intercostal muscles and diaphragm had not the same power, no person could go to sleep, and if the will had not the power of increasing

their action according to circumstances, the motions of respiration could not be rendered more frequent, to keep pace with the increased flow of blood towards the heart, which takes place upon its being hurried forward in the veins by the pressure of the muscles in all parts of the body when they are in action; nor could the greater quantity of blood which then passes through the lungs in a given time be properly exposed to the atmospheric air; consequently no person could take any active exercise without suffering from the intense distress, to which those individuals are liable, in whom, on account of a malformation of the heart, venous blood has access to the arteries.

If any person will pay attention to what occurs when he interrupts his respiration, which may be done either by fixing the ribs, or by closing the glottis, (neither of which can be done without an exertion of the will,) he will find that the usual motions are resumed as soon as the influence of the will can be no longer kept up, and that *respiration is continued only when volition ceases*. Again, if a person throws himself on a couch with a full determination to exercise no power of the will upon any muscle, he will find that he is not able to interrupt his respiratory motions at all. Since therefore, the voluntary and involuntary muscles form

two classes, these which are in their usual action involuntary, and yet are at all times subject to the will, form a third class, which comprehends, besides those already mentioned, those of the throat, abdomen, the levatores, sphincter ani and perinæum. It is denied however by Dr. Philip that there are any muscles of this nature. In a note at page 276 of the *Experimental Inquiry* he says. "There is no such muscle, as far as the author is capable of judging, as what physiologists call a mixed muscle, that is, one partaking of the nature both of the voluntary and involuntary muscles. All the muscles referred to this head are muscles of voluntary motion, but the nature of whose function is such that we are frequently obliged to call them into action." This opinion has led him to give the following definition of a voluntary muscle. "It has been customary to speak of the muscles of respiration as at least in part muscles of involuntary motion. What is meant by a muscle of voluntary motion? It is a muscle, whose action under all ordinary circumstances, we can excite, interrupt, retard, and accelerate at pleasure, but it is not a muscle whose action, we can at all times controul."*

It appears to me that this definition has been given without any regard to the consideration of

* *Philosophical Transactions*, 1829, p. 271.

what extent of power the will has over purely voluntary muscles. This power is, as has been already shewn, that of exciting them and determining the degree of their action necessary for the accomplishment of any given purpose, but is absolutely null in preventing or controuling them when excited to action by any other stimulant; and also that the will has no power whatever of even interrupting the action of any muscle, unless that be called interruption which excites them to the highest degree of action of which they are capable, and maintains them for a short time at that point of contraction. When therefore we observe certain muscles performing constantly repeated contractions which the will has no power of controuling, and that they continue to act thus for an indefinite length of time, it is fair to conclude that there is an essential difference between these muscles and those which never act except when excited by volition.

I cannot help thinking that if Dr. Philip had been less strongly biassed in favour of the doctrines of Irritability, the observation which he makes in a note at page 161,* after mentioning the communications of “the higher spinal nerves with the ganglions of the neck;” “Here we have reason to believe a double communication takes

* Experimental Inquiry.

place, the spinal nerves conveying to the sympathetic the influence of the spinal marrow, and the sympathetic sending with them to the parts to which they are distributed filaments conveying the influence of the ganglionic system," would have led him to a very different conclusion respecting the muscles of respiration.

The above definition of what Dr. Philip considers as constituting a voluntary muscle is the basis of his theory of respiration, the main points of which are, that every act of inspiration is purely voluntary, or the result of an express exertion of the will, and that for the performance of this act it is requisite that the sensorial, nervous, and muscular powers must be combined. Now provided the observations contained in this and the second and third chapters are correct, the following objections may be urged against this theory: In the first place the definition of a voluntary muscle is erroneous—Secondly, it is very difficult to say precisely what is meant by the term "sensorial power" but it is certainly intended to include volition and sensation, which bear an opposite relation to each other, the one acting, the other being acted upon. As these two properties of the brain are entirely distinct from each other, they cannot be so compounded as to form a single power; therefore there cannot be any such

power as that which Dr. Philip has termed "sensorial." Thirdly, the nervous power having been mistaken for that of the will, which forms a part of the sensorial power, the nervous and sensorial powers have been confounded together. Fourthly, the muscles have no power separate and distinct from that which is bestowed upon them by their nerves, therefore the "muscular power," in the sense in which it is used by Dr. Philip, has no existence. It thus appears that the most important of Dr. Philip's errors are brought as it were to a focus in his theory of respiration. Independently of these objections there is yet another contained in the fact, that the motions of respiration do not cease upon the division of the pneumogastric nerves. This circumstance is alone sufficient to shew the fallacy of a theory which rests upon the opinion that each individual act of respiration results from an exertion of volition; because as these are the only posterior nerves by which the lungs are provided, when they are divided, there is no other channel by which the brain can receive any impression from these organs; consequently an animal loses all wish to inspire, and respiration ought to cease, according to Dr. Philip's views, at the moment of the division of the pneumogastric nerves. Again, if this function were purely voluntary, an animal thus injured would

be observed to take frequent and long drawn inspirations, in order that by exerting his will he might endeavour to relieve his distress. Since however the motions of respiration continue when no sensation in the lungs can be any longer conveyed to the brain, or "sensorium," each inspiration cannot be the result of a distinct impression so conveyed, by which volition is excited, and consequently cannot be purely voluntary. The conclusion which I am disposed to draw from these observations is, that all ordinary respiration is involuntary, but the will has the power of increasing the action of the muscles by which it is performed whenever the state of the circulation renders such increase necessary.

The cause of this peculiarity in the action of these muscles is not to be either found, or sought for in any difference in their structure, any more than in that of the purely voluntary or the purely involuntary muscles, but in the nerves by which they are supplied. It will be found upon investigation, that the third class of muscles is supplied by the fourth order of nerves, that is to say, their involuntary action is provided for by means of branches from sympathetic ganglia, they are subject to the will through the anterior nerves, and they have posterior filaments, in order that the healthy condition of their struc-

ture, upon which their fitness to act depends, may be constantly maintained. This order of nerves is very numerous and extensively distributed, comprehending the third, fourth, fifth, sixth, portio dura of the seventh, glossopharyngeal, the primary branches of the pneumogastric, ninth, and the anterior branches of all the spinal nerves, with the exception of the fifth, sixth, and seventh cervical nerves, which form a great part of the brachial plexus. The spinal accessory nerves ought also to be included in this order, on account of the mode of their distribution, and because, as will be presently shewn, they enable the mastoid and trapezius muscles to act constantly without being fatigued, although properly speaking they belong to neither of the four classes. Some of these nerves have been separated into a distinct class, and designated as the Irregular Nerves, and they truly deserve this appellation, if considered merely with regard to their course and distribution; but when we examine in what the irregularity consists, and find that notwithstanding the apparent confusion they all receive in some part of their course sympathetic filaments, by means of which they are supplied partly in the same manner as the heart and intestines, and that the muscles to which they are distributed agree in function, regularity is found in the

midst of disorder, simply because there is a distinct object to be obtained by it: and when it is farther observed that not only those muscles which are concerned in performing the motions necessary for respiration, but that those belonging to the pelvic viscera agree in having a similar mode of action and in being supplied by the same sort of nerves, a doubt I conceive cannot exist respecting the propriety of considering the whole of these nerves as forming a distinct order.

The irregularities which exist in the mode of union of the anterior and posterior filaments with each other, that is to say, the connexion of the third, fourth, sixth, portio dura and ninth with the fifth, glosso-pharyngeal and primary branches of the pneumogastric, are described in every manual of anatomy, as well as those branches which the portio dura receives from the superior cervical sympathetic ganglion, and are too well known to require any particular mention. It is however only lately that the third and fourth nerves have been discovered to receive filaments from the cavernous plexus just before their entrance into the orbit, and that this plexus is connected directly with the great gasserian ganglion. I conceive that this last mentioned communication enables the cavernous plexus to be provided with a constant

flow of *nervous* influence from the gasserian ganglion, in the same manner as the superior cervical sympathetic ganglion is supplied from the protuberance on the pneumogastric nerve, so that all parts to which branches from the cavernous plexus are sent receive a supply of nervous influence. I am farther disposed to believe that the reason why all these nervous filaments, and particularly the direct communication between the gasserian and the superior cervical sympathetic ganglion, are so distinctly pronounced in the calf and in animals, the size of whose faces and jaws bears a large proportion to that of the brain, as compared with that which is observed in the human head, is because, from the large size of the fifth nerve in these animals, there is a greater reflux current of nervous influence returning to the gasserian ganglion, which, on account of the small proportionate size of the brain, is not required to support the action of its arteries, consequently it passes on to the superior cervical. The lenticular, the spheno-palatine, the naso-palatine, and the submaxillary ganglions, I consider simply as sympathetic ganglions supplied from the cavernous plexus by means of their connexions with it; the nerves therefore which proceed from these bodies belong not to the fourth, but to the third order, while the peculiarity of the motor func-

tion of the muscles of the eye and face depends upon the sympathetic filaments which their anterior nerves receive; the secernent function, upon which the nutrition of them and of all the neighbouring parts depends, appears to be maintained chiefly by the flow of *cerebral* influence along them which passes to the extremities of the posterior portion of the fifth. This view of the mode in which the secernent function of these nerves is carried on, enables us fully to comprehend why the posterior portion of the fifth nerve is so large, for though at first sight it appears out of proportion, yet in reality the aggregate of the anterior filaments is equal to them. There can be but little doubt that the same disorganization of the eye, gums, &c., would be the consequence of the division of all the anterior as resulted from the division of the posterior within the cranium, as performed by M. Majendie, because Sir C. Bell has noticed that opacity of the cornea is one of the consequences of the division of the portio dura, from which we may infer that the secernent function was only partially put a stop to by this injury, and which was plainly seen in the cornea on account of its beautiful transparency, when that process which maintains its healthy structure is adequately supported.

I have considered all the muscles of the eye

as belonging to the third or mixed class, both on account of the nerves by which they are supplied, and on account of the nature of their action, but as Sir Charles Bell regards that of the oblique muscles as purely involuntary, it is necessary to take notice of the experiment which he performed to substantiate his opinion. After the division of the superior rectus muscle of the eye of a rabbit, the eye was observed to be turned down more than the other, but, upon lifting up the eyelid, and touching the globe, it was rolled in such a manner as evidently to indicate an action of the oblique muscles. This experiment, as far as I am capable of judging, appears to prove directly the reverse of the position which Sir Charles endeavours to establish, because it is quite clear that the nerves of the eyeball being uninjured, the animal must have been aware of the irritation, and, wishing to get rid of it, would *voluntarily* make use of the muscles which roll the eye for that purpose. This explanation must I think be admitted, when it is perceived that the experiment contains no evidence to shew that the motion was not voluntary.

Considerable attention has been paid to some of the nerves of this order in consequence of the publication of Sir C. Bell's opinions respecting those which he has termed "respira-

tory." The basis upon which these peculiar opinions rest, appears to be the idea of there being a certain part of the superior portion of the spinal marrow expressly devoted to the regulations of the motions of respiration, "a tractus respiratorius," and that every nerve belonging to this tractus is especially destined to combine and assist in the performance of respiration, as a necessary consequence of its connection with that particular part.* I have found it impossible to adopt these views for the following reasons: First, it is by no means clear that the tractus respiratorius has any existence: secondly, some of the nerves termed respiratory arise from the anterior, others belong to the posterior, while the phrenic belong equally to both columns of the spinal marrow. These facts are altogether subversive of the idea of their arising from any one particular part; lastly, in classing the pneumogastric as a nerve of motion, Sir Charles is at variance with the results of his own experiments, which shew that no posterior ganglionic nerve has any thing to do with motion.

Sir Charles Bell is also of opinion that these nerves possess a higher degree of vitality than others, which he considers supported by the

* Exposition of the Natural System of Nerves of the Human Body. London. 1824.

following experiment. At page 213 of the above mentioned work, he says, “ After dividing the spinal marrow between the vertebræ of the neck and those of the back, respiration is continued by the diaphragm; which experiment, as it is often mentioned by physiologists, the author has not thought it necessary to repeat, but only to institute the following experiment on an ass. The phrenic nerves being first divided, and then the spinal marrow cut across at the *bottom of the cervical vertebræ*, respiration was stopped in the chest; but there continued a catching and strong action at regular intervals in the muscles of the nostrils, face, and side of the neck. The main part of the apparatus of respiration was stopped, but these accessory muscles remained animated and making ineffectual endeavours to perform the respiration. When apparent death had taken place, the ass was reanimated by artificial breathing, and then these muscles on the face and neck were restored to activity, and became subject to regular and successive contractions, as in excited respiration, whilst the chest remained at rest. These actions continued for a short time, and then ceased; but upon the artificial respiration being again produced, the same results followed. This was repeated several times, the animal remaining insensible during these experiments

“ Upon stimulating the nerves after the death of this animal, it was observed, that the class of respiratory nerves retained their power of exciting their respective muscles into action long after the other nerves had ceased to exert any power; they were evidently of that class which retain their life the longest.”

It will be observed that in this experiment, the spinal marrow being divided *below* the origins of the “ respiratory nerves,” they all, except the phrenic, remained connected with the brain, consequently, if the nerves derive any influence from that organ, the muscles supplied by them would naturally continue to give evidence of the communication being uninterrupted. This experiment therefore proves, that those nerves, whose connection with the brain remains entire, retain their vitality longer than those which have been separated from it, but nothing more; because, in order to prove that “ the respiratory nerves” retain their vitality longer than other nerves placed in a similar condition with regard to the brain, it should be shewn that the motion of their muscles would continue longer than those of other muscles after the spinal marrow had been divided at the occiput. It is not mentioned whether it was ascertained that the peristaltic motion of the intestines had ceased.

The only nerves which yet remain to be noticed particularly are the Spinal Accessory nerves; but though last, they are among the most important as regards the proof they afford of the existence of the *Circulation of the Nervous System*. The shortest way of shewing their function will be to adduce the following decisive and satisfactory experiment which was performed by Sir C. Bell, and is related by him in his work on the Natural System of the Nerves already referred to. “In the ass there are two muscles which take the office of the mastoid muscle: one is inserted into the jaw, which we may call *sterno-maxillaris*, and the other into the vertebræ, viz. *sterno-vertebralis*. To these the superior respiratory nerve (or spinal accessory) is distributed in its passage to the trapezius. These muscles are at the same time supplied with numerous nerves directly from the spinal marrow. If we expose the superior respiratory nerve, and then induce excited respiration, so as to bring these muscles into powerful action in combination with the other muscles of respiration, and if while this action is performed, we divide the nerve, the motion ceases and the muscles remain relaxed *until the animal brings them into action as voluntary muscles*.”

Without entering into any speculations re-

specting the respiratory functions of these nerves, it is sufficient to know that they bestow a power of involuntary motion upon the mastoid and trapezius muscles. If any farther evidence were required to shew that the ganglions do not interrupt or break off the influence of the will from affecting the nerves beyond them, it is afforded by the accessory nerves which have no ganglion, and yet cause the muscles supplied by them to act involuntarily. Neither can the doctrine, which teaches that the action of muscles is involuntary because their stimulus is so applied, be brought to bear in explanation of the action of the mastoid and trapezius, as it has been to explain that of the heart and intestines, for there can be no analagous stimulus to excite them; but there is a great necessity for their having a power of involuntary motion to enable them to act constantly without becoming fatigued in moving and supporting the head, a function which muscles, supplied only by anterior and posterior nerves, would be by no means equal to.

It is now to be observed that the spinal accessory differs from all the rest of the nerves which belong to the posterior columns of the spinal marrow, in several points. First, the most obvious difference is in the direction of the course of their filaments, which is from below upwards. The second is in their having no

protuberance or ganglion upon them either before, or at their emergence from their bony canals. The third, in being motor nerves. They farther differ from the rest of the posterior nerves in that, as the motor power which they bestow upon their muscles must be derived from the spinal marrow, *they arise from*, while the latter *terminate in* the posterior columns. The spinal accessory nerves must therefore be regarded as bearing the same relation to the posterior columns, which the anterior nerves bear to the anterior columns of the spinal marrow. These are the peculiarities which have prevented me from considering the spinal accessory as belonging to the third class of nerves, and at the same time induced me to place them in the fourth order. The only point which now remains to be explained is how they receive the influence which they bestow upon the muscles. The direction of the course of their filaments precludes all idea of their influence being directly derived from the brain, even if the fact of their being involuntary did not discountenance such an opinion. It must therefore come from the posterior columns of the spinal marrow, and unless there is an upward returning current along these columns towards the cerebellum, the physiology of these nerves is absolutely inexplicable; but if there is such a current as that which I have endeavoured to

shew the nervous system is well adapted to carry on, it is precisely that which is most admirably calculated to provide a supply of nervous influence; constantly, because the action of the brain is constant; and involuntarily, because the will can have no power over the nerves along which it flows. According to this explanation, all the difficulties which have hitherto appeared to present insuperable obstacles, and to baffle every attempt to elucidate the physiology of the spinal accessory nerves, actually combine to afford the most beautiful confirmation of the existence of the circulation of the nervous system, which could possibly have been imagined.

I have thus endeavoured to state in a plain and simple manner the greater part of the facts and inferences which have induced me to believe that there is a circulation in the nerves, and to shew that instead of there being several separate and independent Vital Powers, there is but ONE, upon which every function necessary to the continuance of life depends; so that all the various phenomena result not from the exercise of different powers, but from a difference in the mode of the application of one and the same power.

There is but little evidence by which we may be guided in judging of the rapidity with which the nervous circulation is carried on,

but we know that it is not so great as to prevent the voluntary muscles from becoming weary, as also that when they are fatigued, they are required to be a considerable time at rest before their power is perfectly restored. The ordinary degree of rapidity may be, I think, inferred from observing what occurs in a limb whose nerves have been pressed upon some time, and which has become benumbed, or, as it has been more commonly called, asleep. When the pressure is removed a sensation of tingling, vulgarly called pins and needles, is quickly perceived, and it is not until after the cessation of this sensation that the muscles are again completely qualified to act in obedience to the will. The same sort of tingling, though in a slighter degree, may frequently be perceived in the legs and feet after an unusually long walk, if the attention be directed to it, and also in the female breast during the secretion of the milk. The fact of our being sensible of the tingling, distinctly proves the participation of the posterior nerves in the process by which the power which enables muscles to contract is restored; and as this power cannot arrive at the muscles from the brain except along the anterior nerves, I consider this fact as affording strong evidence of the passage of the *Cerebral Influence* from the extremities of the anterior

to those of the posterior nerves, and consequently it becomes a confirmation of the opinion that there is a circulation in the nerves. Although it cannot be precisely determined with what degree of rapidity this circulation is carried on, there is great reason to believe that it varies more or less in nearly every individual, of which the pulse is the index; for inasmuch as the circulation of the blood is dependent upon that of the nervous system, it must always bear a certain relation to the vigour and activity of the power by which it is supported. To this cause I am disposed to attribute the numerous peculiarities of constitution observed in different individuals, and the reason why they are propagated from generation to generation. The high state of activity with which the whole secernent function is performed in children and in young animals, shews that the nervous circulation is carried on with far greater rapidity in them than in adults. But whatever this may be when the mind is undisturbed, it is at all times liable to be accelerated or retarded by any thing which has a corresponding effect upon that action of the brain, which causes the flow of cerebral influence from it, as may be seen when persons blush or grow pale. The face is the part where evidence of increased action is first apparent when caused

by a mental emotion, because the nerves by which it is supplied are those which are nearest to the brain, and for the same reason the redness extends down the neck, bosom and shoulders more or less rapidly according to the intensity of the emotion; which also determines whether the increased action of the vessels shall be slight and transient, or continue until it terminates in general perspiration. The colour which mantles upon the face in the act of blushing has hitherto been supposed to result from a direct and primary action of the brain upon the heart, by which the flow of blood through the vessels is suddenly accelerated. There are however two well marked circumstances which prove that the effect upon the heart is only secondary, viz. the redness of the face is always evident before the increased action of the heart is perceptible, and the progress of the blush, instead of being in a direction from this organ to the face, i. e. from below upwards, which it would be if the heart was primarily affected, extends in exactly the opposite direction, or from above downwards. The supernatural strength of those individuals who are under the influence of the more powerfully exciting emotions, as for instance that of rage, and the difficulty with which they are restrained, clearly demonstrates that the power

of the voluntary muscles is as much increased as that of the heart. The same disposition to action with an evident increase of strength is always attendant upon those whose brains are artificially excited by wine or spirits, these facts however are too well known to require that more than the cause of them should be pointed out. The effect of the depressing passions is precisely the reverse of that which is caused by increased action of the brain, and the paleness of fear is also, for the same reason, apparent in the face before the affection of the heart can be perceived. Nor is the enfeebling effect of fear upon the muscles less decidedly marked than the contrary one of rage. This fact also is so familiar to every one, that "paralysed by fear" is a common expression. The sudden debility thus produced cannot result from a loss of the power of the will, because that must under all circumstances invariably remain the same, but from the muscles being unable to act with vigour in obedience to it, on account of the diminution of that influence, upon which alone their ability to contract depends.

Two affections which are opposed to each other have here been instanced on account of the contrast they afford; but if the one increases and the other diminishes the action of the brain, and consequently the flow of cerebral

influence along the anterior nerves, every possible combination of emotions will produce its corresponding effect upon the system, which however must necessarily vary in every individual according to his natural disposition. Need we then be surprised at occasionally meeting with instances in which immediate death has been caused by the too sudden communication of the extremes of either good or evil fortune, or that in such cases, individuals should have died more rarely from the former, than the latter cause?—From these observations it appears that we have little or nothing whereon to ground an opinion respecting the ordinary activity of the nervous circulation, because being like that of the blood, independent both of volition and of sensation, we are not conscious of it, except when it is disturbed in some way or other, consequently we can only judge of it by its effects then made manifest. This is fortunately a point of little importance when compared with that of establishing its existence, for since every vital function is dependent upon it, whether fast or slow, it must be sufficiently rapid to maintain them all at their healthy standard.

I am not so sanguine as to imagine that the opinions which I have endeavoured to establish will be readily acquiesced in, both on account of

their novelty and because the investigation necessary to determine whether they are right or wrong requires both time and labour. But however unprepared the minds of my professional brethren may be to receive them, let it not be supposed that they were either hastily or inconsiderately formed, for no one could have entered upon the study of physiological experiments with a mind more free from prejudice than myself. I was indeed, as every one else must be, aware of the many discrepancies of opinion on almost every important physiological point, existing between men of great and nearly equal reputation; but the only opinion I had then formed, was that all of them might possibly be more or less right or wrong, and that the only mode of distinguishing the one from the other, would be to examine the facts from which their conclusions had been drawn. In the midst of the difficulties in which I found myself immediately involved, I looked around for the most simple and incontrovertible facts respecting the functions of the nerves upon which all parties were agreed, and it appeared to me that the separate and distinct functions of the anterior and posterior nerves, as demonstrated by the experiments of Sir C. Bell and of M. Majendie, were exactly what I wanted. These facts I considered as the more likely to

lead me in the right path, because they were unknown to the older physiologists, and since their discovery no one had taken advantage of them.—I therefore determined that they should be my guides through the labyrinth of contradiction. The result has been that the opinions which are contained in the preceding pages were, as it were, forced upon me as I proceeded, and I could not persuade myself of their truth until I had searched for and examined every experiment and fact which appeared likely to afford evidence of their futility.

If any one should be disposed to refuse his assent to this newly discovered function of the nervous system, from a supposition that had it existed, it could not have remained so long unknown; let him in the first place recollect how short a time has elapsed since the discovery of the distinct offices of the anterior and posterior nerves has been made, without which it is quite impossible that the nervous circulation ever should have been developed, and that since these facts were made known by Bell and Majendie, no one has perceived the use which may be made of them in explaining and throwing new light upon the hitherto hidden functions of the nervous system: they have only been regarded as simple facts well ascertained, and certifying what had been before surmised,

but not as leading or tending towards the developement of any thing with which we were previously altogether unacquainted. In the second place, let him search diligently whether the whole range of physiological experiments which the ingenuity and industry of our predecessors have accumulated, affords a single fact which contradicts the possibility of the performance of the function which has here been attributed to the nerves, or which proves it to be incompatible with their more obvious functions relating to volition and sensation. Any one who will undertake this examination must observe a circumstance which is of no small importance, as a corroboration of my opinions; namely, that all Dr. Philip's experiments not only admit of being explained by, but actually support opinions entirely at variance with those they were brought forward to substantiate. To this there is but one exception, which, as is generally the case, only tends to confirm the rule. I allude to that experiment in which it is attempted to be proved, both in the frog and rabbit, that the heart's action may be increased by stimulants applied to the anterior part of the brain, after the posterior part has been removed and all medullary and nervous communication between these two organs has

been interrupted. The reason why this exception is to be regarded as a confirmation of the general rule, is because it has been already shewn that sufficient attention was not paid to circumstances connected with the performance of the experiment, which was necessary to ensure an accurate result.

Dr. Philip is one of those who commenced his researches under the disadvantage of being ignorant of the distinct functions of the anterior and posterior nerves, and had fully made up his mind upon all the leading points before this discovery was published to the world; consequently he was naturally less alive to the perception of the new views which these new facts unfold. It was not therefore to be expected but that in his conclusions some truth should be found interwoven with many errors. In the unsoundness of Dr. Philip's doctrines we have an excellent example of how far a person may wander from the right path when the first devious step has been taken; and if this is the reason why in his work on the Vital Functions he occasionally hovers so near the truth, yet fails to find it, it is for the same reason that those who have hitherto objected to his conclusions have been unable to discover precisely wherein the errors consisted; still the great

body of facts which that work contains will always remain valuable, and an honorable monument of his industry, when the faults have been forgotten.

THE END.

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